Expert Review Comments on the IPCC WGIII AR5 First Order Draft – Chapter 6

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12621	6					I am concerned by the heavy reliance of BECCS in teh overshoot scenarios without any analysis of the availability of such significant amounts of sustainable biomass.	Accepted. We intend to better highlight the role of BECCS and associated issues in the SOD
12664	6					I am concerned by the heavy reliance of BECCS in teh overshoot scenarios without any analysis of the availability of such significant amounts of sustainable biomass.	Accepted. We intend to better highlight the role of BECCS and associated issues in the SOD
6401	6					In my version, this figure is really screwed up. Regardless, I'm not sure how much this shows. The x-axis is "degree of international cooperation" but it appears to really only show two locations on the x-axis that are close to each other.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
6394	6					Is the CO2 budget in GT?	The figure shows total radiative forcing from all agents (CO2, other gases, non-gas agents such as aerosols) in Watts per square meter.
6404	6					I really like this figure. But I think it needs more description.	The author team is working on ways to clarify the description of this graphic.
6405	6					It is not clear what Low30 and High30 refer to.	The author team is working on ways to improve this graphic.
6406	6					The figure is screwed up.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
17277	6					The way of presenting the ranges has become more or less the standard in IPCC report. It provides median results with a band-with that looks like an uncertainty bandwith. However, it hides that all these are individual modelling results that show great variety. In the text, more attention should be paid to explaining the differences in outcomes between the various models.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. This includes a revision of figure formats that more adequately represent the fact that a large number of studies are included in such summary figures. Also pointing out specific assumptions of studies that lead to certain developments (e.g. high/low emissions or energy use) will be pointed out to the degree possible.
17278	6					Next to presenting ultimate emissions, it would be also useful to present emission reduction efforts compared to a reference development.	Comment is noted and will be considered as the text is adjusted for the next draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2421	6					I found the discussion of SRM and CDR, although necessary, to be piecemeal. It is not clear for instance why SRM is discussed in two different places. My suggestion would be to discuss negative emissions and BECCS in section 6.3 because these are already considered in some of the existing stabilisation scenarios but defer the discussion of SRM to section 6.9. This separate section on geoengineering should rely much more on the assessments already made in WGI (chapter 7) and WGII and focus on aspects that are directly relevant to WGIII. What is the potential for CDR (beyond BECCS) and SRM to modify the existing framework of stabilisation scenarios? The comment on cost-benefit vs risk-benefit is interesting, but can nothing more be said in that respect? Can we / should we differentiate temporal scales when discussing SRM (ie a few decades vs a century vs several centuries)? The termination issue needs to be discussed in the context of stabilisation scenarios.	Noted. Although it might be valuable to split the discussion of CDR and SRM into two subsections, the outline for the section has already been approved and it would be difficult to change at this time. The SOD will discuss CDR and SRM implications for emisisons pathways in 6.3 and then discuss technologies in 6.9.
2182	6					Thetitle of the Emissions level relative to 2005 should give units (%).	Editorial
13761	6					What is % NPV? You mean NPV as % of GDP? The etire following discussion of this aspect is confusing	Taken into account. The text and figure are adjusted.
13135	6					This is over 400 scenarios, how many models have been used for creating these? Are all models approximately similarly represented, or are some models strongly overrepresented? Elaborate here or elsewhere (section 6.2.5?), so that the intra vs inter model differences become clearer. Would be good to have statistics on models vs targets (i.e. a matrix that shows climate categories on one axis and model names in another, then populate with numbers of targets run by given models), I think this would be very important for understanding the sources of variation.	We will add a statistics on model. Overrepresentation of certain models is indeed an issue.
13148	6					What explains the below one index for cat 1 mitigation costs for the partial equlibrium models? Is it because most (in comparsion to the cat 3 scenarios) of their costs occur post-2050?	Taken into account. Figure revised.
13155	6					Figure not readable.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
13158	6					See previous comment, I suggest the figure is removed.	Figure 6.24 has been replaced by a figure that relates climate targets to the use of fossil fuels to link the discussion of climate targets with that on fossil fuel scarcity.
13160	6					See the previous comment, I suggest the figure is removed and replaced with a short explanation. More detailed results on low carbon technologies follow in any case. If it seems absolutely necessary to include a figure, I'd rather see a single figure showing (for 2050) the low carbon share (or absolute numbers, if this otherwise get too close to the figures that follow later) on one panel and total primary energy in another.	Figure 6.25 is kept in the section to illustrate that the absolute level of low-carbon energy deployment is not fully determined by the climate target. A share figure would not make this point.
13161	6					This figure, while very nice in theory, is not readable in its current form. The letters, especially, are nearly impossible to read, make the figure very messy and I would therefore strongly recommend removing them (it's also impossible to read any colours for the letters). An option might be to just indicate the base year and final year of each transition path (with climate target dependent markers?), so that the time element could be kept in the figure.	Figure will be reworked.
13162	6					As with the previous figure, the letters are not visible and make the figure messy. I don't think they're really necessary either, as it doesn't seem that important to know which model has produced a certain path. Finally, might be worthwhile to consider some alternative division of fuels as currently most of the figure is empty (because solids having such low shares on the end use level).	Figure will be reworked.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13163	6					Also here I would suggest removing the letters and replacing them with simple markers. Especially if it's expected that more scenarios will be included for the SOD.	Figure will be reworked.
13165	6					The definitions for the technology variation scenarios need to be explained already here. Currently they are included in the following figure (6.30)	Figure 6.29 has been removed due to space constraints. However, an explanation of the numbers at the bottom of the figures has been added to the figure caption of Figure 6.30.
13168	6					This figure is mostly empty space and therefore mainly communicates that there are a couple of outlier scenarios in which land use related CO2 mitigation is especially strong. If this is not the only message that the reader should get from this figure (especially the left panel), I would recommend altering the figure.	Figure revisited.
13169	6					I don't think it's necessary to show explicitly the results for individual models; the figure gets very messy and difficult to read. Show specific ranges instead, for the three milestones years and for the two scenarios.	Figure revisited.
13171	6					This figure doesn't seem very necessary: The logic of the emission trajectories doesn't differ significantly from those of delayed participation scenarios (which are included in the previous figure), even if they are produced differently (and may have more/less optimal emissions in the short term). Add also these scenarios to figure 6.34, or alternatively create two figures, one with full where and when flexibility and one with non-optimal mid term emissions (delayed participation, myopic, stochastic etc scenarios)	The author team is working on ways to improve this graphic and better distinguish it from the previous figure.
13173	6					Figure not readable.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
13185	6					Figure not readable.	Accepted. Replaced with other figure.
13189	6					Cross reference this with what's said on page 45, lines 19-22 (and shown in figure 6.26?). Presumably the difference could be explained by the different time horizons (2100 vs 2050) and/or by significant differences in total final energy use between the climate categories? Please clarify, in any case.	A better integration of Section 6.2.7.2 (in SOD 6.3.4.2) and Section 6.8 is planned, including cross-referencing between the two sections.
13138	6					How do you explain the wide range of cat 4 cumulative emissions for 2000 to 2100? The range, in terms of forcing, should be only 1 W/m2 for this category (4-5 W/m2), but the cumulative emissions can almost triple and climate consequences would still be consistent with cat 4? Surely non-CO2 gases alone can't explain this and the significant overlap with cat 5? Please elaborate.	Differences in carbon cycle representation of models; timing; etc. We will elaborate the overlaps for sure.
11246	6					· I am missing something like “no regret options”. This could be an interesting framing, e.g. no matter which delay we face, no matter which stabilization target should be achieved, technology X is always important and is required at a deployment level of Y.	Noted. We have not decided whether to use the notion of "no-regrets" options as part of the framing of the story in the chapter. We are considering alternnative framings for explaining the sorts of actions that seem to be found across scenarios.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11260	6					Fig. 6.10 is one of the key figures for me, this should come in the executive summary, because it shows the effec of CCS and Bio+CCS availability on the emission profiles. If a figure with the new model results can be provided where one can see from one model the differences of “w CCS”, “w/o CCS” and “w or w/o bio-CCS” this would be extremely interesting	We will try to do so.
11266	6					Fig. 6.13: I cannot agree to the message that “the costs are highly dependent on the level of stabilization”. From Fig. 6.13 I can only see that there is a slight, but mainly linear increase. Only CGEs are differnt, this should be explained. The x-axis is probably misleading with equidistant part between the categories	Noted. Now the figure provides separate bars for different cost metrics and the text is adjusted.
16690	6					When discussing "transformation pathways" are we talking about transformation of the energy system technologies, or transformation of the emissions trajectory over time? This is sometimes unclear.	Noted. The next version will at least introduce the notion of a transformation pathway.
9066	6					premature to comment as results are still preliminary	Noted
10970	6					This is the best of the four chapters that I have read.	Noted.
17476	6					what do the diagonal lines represent?	The lines have no meaning and were not part of the original document. There seems to have been a conversion error in the preparation of the FOD.
17477	6					what do the diagonal lines represent?	The lines have no meaning and were not part of the original document. There seems to have been a conversion error in the preparation of the FOD.
17807	6					The transformation pathways does not take into account an indepth social and political analysis - linkages with chapter 3 are missing and societal issues that could be dealing as drivers	Noted. Efforts will be made to reference Chapter 3 as appropriate.
17808	6					are not described.	Nonsensical
13737	6					Figure 6.12 should differentiate solar radiation management (SRM) and carbon dioxide removal (CDR) rather than putting the umbrella term Geoengineering twice in the chart, which confuses the reader even more as one needs to figure out what type of technology is meant. In Addition 'sequestration technologies' is inacurate here: If it said carbon dioxide removal (CDR) it were more clearly differentiated from both CCS (which belongs in the Mitigation technologies category then) and from SRM (which interferes in the link denominated C in the graph).	Taken into account--figure and text deleted
8100	6					This figure contains a very strange choice of percentile ranges for which no argumentation is provided. Suggestion: use 90% range (5 to 95 percentile), interquartile range (25 to 75 percentile), and median	We will harmonize this in the chapter
3047	6					The model comments below may apply to some or all of the models listed in Table 6.1, but the model list differs from that in Table 1.8 and documentation for the new models listed in Table 6.1 is apparently not available for this review cycle. Accordingly, comments are listed as being attached to Chapter 1.	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2981	6					As far as I know, Message is a partial equilibrium model (instead of a GE model). In addition, the issue of dealing o intertemporal optimization is quite complex in it. Please see Ilkka Keppo and Manfred Strubegger (2010). Short term decisions for long term problems – The effect of foresight on model based energy systems analysis. Energy 35, 2033-2042 . Actually, as precisely discussed in this paper, one major challenge behind optimization models simulation is to incorporate uncertainty and asymmetric information to their run. As Keppo and Strubegger said, "While the traditional optimization framework provides the globally optimal decisions for the modeled problem, the framework presented here may offer a better description of the decision environment, under which decision makers must operate… the operation of the model is demonstrated using a moving window of foresight, with which decisions are taken for the next 30 years, but can be reconsidered later, when more information becomes available. We find that the results demonstrate some of the pitfalls of short term planning, e.g. lagging investments during earlier periods lead to higher requirements later during the century. Furthermore, the energy system remains more reliant on fossil based energy carriers, leading to higher greenhouse gas emissions…".	Noted
2989	6					Why does deployment in 2010 present a range of values, being it a data and not a forecast? Figures for oil, nuclear, coal cannot show this range of uncertainty for 2010.	Noted. The ranges shows the model spread in this year.
2985	6					It is not clear	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
2987	6					It is not clear	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
2988	6					It is not clear	Accepted. Replaced with other figure.
14449	6					Overall Chapter 6 is well organized and clearly written.	Noted
14450	6					Overall Chapter 6 figures and tables are well formatted.	Noted
10398	6					The categories about the characteristic of IAM are very good but it loses some important items such as whether climate feedback to economy exits and how the technology progresses, ect.The table 6.1 mainly contains models of gerneral equilibrium models. Gerneral equilibrium models such as CGE models have a great advantage to show deitals in sectors and regions, however, it is difficult to reflect the innovation of technology progress and difficult to combine with the carbon circle system and as said in this section, CGE models lack of foresight of the level of investment in the long run. As the problems listed above exist, macroeconomic models as RICE2007,DICE2010,MRICE(Wang, Zhang, Wu, 2012) have great advantages in dealing with those problems.I recommend to biuld another table incorporating these important models to make the categories better.	Taken into account--section revised and details eliminated. Table deleted due to space constraints

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15427	6					This figure is confusing. First, the graphic has the effect of suggesting that geoengineering IS a climate change response strategy on par with mitigation and adaptation. It is fundamentally flawed and unethical to treat adaptation on par with geoengineering vis-a-vis mitigation and in the overall policy landscape -- i.e., to play down real, on-the-ground adaptation strategies. Adaptation is the only option for many developing and least-developed countries, and geoengineering has a highly debatable role in the overall climate policy anywhere. Though one point of the graphic seems to be that geoengineering would compete with R&D and investment resources, the "competition" aspect is not apparent in the graphic, as is -- it appears as if the point of the graphic (and the accompanying text) is to elevate geoengineering to the level of mitigation and adaptation -- which would be a radical and controversial position for the IPCC to take.	Taken into account--figure and text deleted
9957	6					Please specify the units for items in Table6.2.	Will be added
9959	6					Units are missing. And please introduce the economic and technological projects in each model, so that readers can understand well what the role of radiative forcing play in emission pathway.	Will be added
9954	6					Unlike Figure 6.7 there is a range for emission in different RCP scenarios, the emission is definite in this figure, so based on which model you get the trajectory or just an average number based on models AR5 adopted.	The data shown in the figure is from the published RCP results (Van Vuuren et al, 2011). This reference will be clarified in the chapter.
9961	6					When compare the pathway without BECCS in Figure 6.10 with pathway of category 1 in Figure 6.7, it can be found that negative emissions don't occur in Figure 6.10. Does it mean that there are BECCS for emissions reduction in pathway in Figure 6.7? So maybe it's necessary to make it clear which reduction measures are taker for pathways in Figure 6.7.	We will clarrify this.
9956	6					The target stabilization level is not demonstrated in Figure 6.2.	The data shown in the figure is from the published RCP results (Van Vuuren et al, 2011). This reference and the figure caption will be clarified in the chapter.
9958	6					This figure is intersting to show emission pathway in different categories, but since emission reduction measures, which are implict to reach such pathways, are unknown. It is believed that any IAM can produce such pathways, but the feasibility must be focused on otherwise those pathways just don't make any sense.	There is a discussion on the word feasability in the context of models in the tekst. The technologies are discussed further in the Chapter
12607	6					Would be good to write on the text the concept of transformation pathway	Noted. The next version will at least introduce the notion of a transformation pathway.
8101	6					The description of the scenario ensemble used in the assessment is lacking (as also indicated by the Authors). Because much of the transformation pathway discussion, and in particular sections 6.3.2 and 6.4, will depend strongly on how scenarios were selected and constructed, it is critical that the scenario ensemble is clearly described and its limiations highlighted in sections 6.3.2 and 6.4	Accepted. The scenario ensemble will be described early in the chapter.
13191	6					Considering the enormous uncertainties of such an economy wide, decades long transitions, I would think there's more "gap" than what we do know for certain. This isn't really a "proper" gap, of course, as it is not possible to acquire information or data that would be able to fully fill this gap. I still think it's important to make this clear, i.e. what is currently listed here are gaps for incrementally improving the modelling of the scenarios, but they will in no way remove the enormous uncertainties that make specific and concrete modelling based, non-obvious recommendations difficult.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3376	6					This is at the core of the AR5 report and should be expanded to allow a more careful interpretation of chapter 6's results. For example, the two following studies detail the limitations of integrated assessment studies with respect to dealing with the uncertainties of future development: A) Ackerman, F., DeCanio, S. J., Howarth, R. B. & Sheeran, K. Limitations of integrated assessment models of climate change. Climatic Change 95, 297–315 (2009). B) Cullenward, D., Schipper, L., Sudarshan, A. & Howarth, R. Psychohistory revisited: fundamental issues in forecasting climate futures. Climatic Change 104, 457–472 (2011). Epistemological challenges related to scenario analysis and uncertainty should not be ignored. Scenario analysis does not follow the scientific gold standard of falsification and there is risk of systematic bias, e.g. due to herd crowding, in e.g. integrated assessments. Discussion of this point seems to be absolutely crucial. One important study on this issue is: "Betz, G. (2009),Underdetermination,Model-ensemble,andSurprises	Rejected--space constraints
10392	6					Because the uncertainty has a clear definition in science, it is not properly to explian the uncertainty by different results across the IAM models.	Noted
5326	6					This section does a poor job in explaining how an integrated assessment model differs from a normal CGE-model. According to my understanding, an important issue of an IAM is that it tries to model, explain and calculate the losses and damages caused by human action via the channel of global warming endogenously.	Taken into account--we have added text on cost-benefit analysis which tries to make this distinction
8102	6					On the one hand this section states that there is no unique definition of greenhouse gas concentrations, on the other hand it doesn't provide yet insight in how the radiative forcing in Table 6.2 was determined. In absence of a unique definition of greenhouse gas concentrations, an explicit definition of what is included in the radiative forcing is crucial. Linking the budgets, concentrations and radiative forcing estimates to the WGI assessment n this issue would make this table much stronger.	Correct. Will be added
8110	6					Also the issues with regard to the time scales of the reversibility of possible temperature overshoots might be interesting to highlight. For example based on: Lowe, J. A. et al. How difficult is it to recover from dangerous levels of global warming? Environmental Research Letters 4, 014012 (2009).	Good point. We will add this.
9963	6					If possible, please give some temperature stabilization pathways as those shown in former sections.	A new section on climate will be written
12542	6					The discussion of SRM is bound to be controversial. This portion in particular is problematic: “Absent SRM, near term decisions may be strongly contingent on the low-probability high-consequence “tail” of the probabilistic distribution of climate sensitivity and climate impacts. Because SRM can be implemented quickly (decades) whereas reduction in concentrations takes place on century-timescales it might, in principle, be implemented after uncertainty is partially resolved. This attribute of SRM makes it valuable in managing climate risk even if the costs and damages of SRM were comparable to the costs of mitigation and the damages climate change.” There is no evidence, only conjectiure, on what time scales SRM can be implemented. It is not logical to claim that an attribute of SRM “makes it valuable in managing climate risk,” since value must relate to evidence-based and not conjectural performance. There may be conceivable pathways toward testing SRM and other geoengineering approaches and evaluating them for deployment without making specific claims as above based on present knowledge. Please stick with those assessments.	Several papers address timescale for implementing SRM.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9965	6					Since carbon cycle and climate is so important that we have to spend more words on introducing more about the details and characteristics of carbon cycle model in IAM. For examples, how many carbon reserviors are included in carbon cycle model in each IAM that AR5 concerned. For your reference, the paper named 'the benefits of climate change mitigation in integrated assessment model: the role of the carbon cycle and climate component'(Hof, 2012) is really good about this issue. I will submit it to the TSU.	We agree and discuss the climate part in greater detail
10397	6					Although consumption is very important to anlysis the economy cost in the view of welfare, GDP is also an important index for economy cost which cannot be ignored because of its strong relationship with employment and its reflection of nations' economy which is the mian concern of most governments.	Taken into acount. Cost figures now show both consumption and GDP changes where appropriate.
11256	6					Evaluation of the costs: there is so much said on the costs in chp. 6.3.4.2. But then comes the surprise in chp. 6.3.7.3 telling us that “mitigation costs are heavily influenced by the nature of the available mitigation technologies”. If this is the case, what is the reason to overstress the cost figures e.g. in Fig 6.13. It should be contrasted directly. By the way, on my option the spread of the costs in Fig. 6.29 is rather low. Only the “no CCS” case is a bit different, but I would not interpret this figure as if the technology choice had large influence. Model differences seem to be much larger.	Noted and will be considered as we adjust the text in the next draft.
2982	6					The section minimizes the uncertainty of crude oil price as one major source for the uncertainties in mitigation scenarios and its costs. The same is valid for the different assumptions found in the scientific literature for supporting the choices of discount rate.	Noted. The role of discount rate is addressed explicitly now.
2983	6					The effect of the learning curve on the evolution of the abatement costs of mitigation options and even on the choice of the least cost path should be highlighted in the document. Please see: Blyth, W., Bunn, D., Kettunen, J., Wilson, T., 2009. Policy interactions, risk and price formation in carbon markets. Energy Policy 37 (12), 5192-5207.Broek, M., Hoefnagels, R., Rubin, E., Turkenburg, W., Faaij, A., 2009. Effects of technological learning on future cost and performance of power plants with CO2 capture. Progress in Energy and Combustion Science 35 (6), 457-480. Rochedo, P., Szklo, A., 2012. Minimum Work of Separation and Learning Curves for Carbon Capture based on Chemical Absorption. To be presented at 7th Conference on Sustainable Development of Energy, Water and Environment Systems, Ohrid, Macedonia.	Noted and will be considered as we adjust the text in the next draft.
2984	6					As I proposed for Section 6.3.4.2 to include the analysis of the effects of learning curves (innovation) on the least cost abatement paths, I suggest including in section 6.3.5. policies oriented toward anticipating investment in abatement options with higher learning rates (i.e. those with experience curves that justify the previous incentivized investment).	The issues related to innovation are considered in other sections.
17284	6					The RECIPE project (Luderer et al., 2012; Jakob et al., 2012) analyzed the implications of delayed and fragmented climate policy. Their results should be included in this discussion.	We are aware of this study and will take into account, though it is already outdated.
11255	6					The sectoral analysis is not shown in a consistent way, it is spread over different subsections in the chapter. Chp 6.3.7. should say something on sectors, but I do not see what chp 6.3.7.3. contributes to this.	The discussion of sectoral developments at a disaggregate level will be taken care of in Section 6.8 while Section 6.3.7 (in SOD 6.3.4) will address the interdependence between energy supply and demand at a more aggregate level (i.e., for all end-use sectors combined rather than by end-use sector).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13172	6					Stochastic scenarios, with uncertain long term targets, should also be mentioned, as they explicitly investigate the relationship between mid and (uncertain) long term targets. Already the TIAM family of models alone has published at least 3 papers on such scenarios and there must be other models that have been used in similar fashion (refereces to the TIAM papers: Syri, Lehtilä, Ekholm, Savolainen, Holttinen and Peltola. Global energy and emissions scenarios for effective climate change mitigation—Deterministic and stochastic scenarios with the TIAM model, International Journal of Greenhouse Gas Control, 2(2), 2008, pp 274-285, ISSN 1750-5836, 10.1016/j.ijggc.2008.01.001. ; Loulou, Labriet and Kanudia. Deterministic and stochastic analysis of alternative climate targets under differentiated cooperation regimes, Energy Economics, 31 (Supp 2), 2009, pp S131-S143, ISSN 0140-9883, 10.1016/j.eneco.2009.06.012. ; Keppo and van der Zwaan. The Impact of Uncertainty in Climate Targets and CO2 Storage Availability on Long-Term Emissions Abatement, Environmental Modeling and Assessment, 17(1-2), 2012, pp. 177-191, DOI: 10.1007/s10666-011-9283-1)	The author team will work on adding references to studies explicitly examining stochastic control and will incorporate the notion into the introduction section.
3629	6					Delete or massively reduce to save space as overlaps with chapters 4.1.3.2, 4.6.1, 4.6.2.	The structure of the chapter is being revised to best cover the material within the prescribed outline. However, we likely need to keep some or most of the material in this section. Every effort will be made to minimize overlaps and redundancies.
2986	6					For most of the technologies I do agree with the idea presented in this section, which expresses that “the benefit of beginning to create and improve technologies today and to develop institutional capacity is that it creates opportunities to make early and midcourse corrections.” However for some options, usually the least mature ones, the choice of a technology route may cause lock in problems. The case of CCS is emblematic. See Markusson, N., Haszeldine, S., ‘Capture readiness’–lock-in problems for CCS governance, Greenhouse Gas Control Technologies, Vol. 1, Issue 1, pp 4625-4632, 2009	We address different types of lock-in but can add a reference here to the specific issue mentioned.
17234	6					The study below is useful in the context because it shows that short-term technology policy helps to overcome the time of delayed carbon pricing. The study shows that short-term investments help to moderate future CO2 prices in achieving a given atmospheric stabilization target. Bauer N, Baumstark L, Leimbach M (2012): The REMIND-R model: the role of renewables in the low-carbon transformation—first-best vs. second-best worlds. Climatic Change, online first. DOI 10.1007/s10584-011-0129-2	We will include the reference.
3630	6					Delete or massively reduce to save space as overlaps with chapter 4.3.4.	We have written this section more concisely, but there was broad agreement in the author team that this section should be kept in this place; efforts are done to avoid overlap with chapters such as no.4.
6411	6					I really like sustainable development and I think it is extremely important. But this section seems to not fit the rest of the chapter. The rest of the chapter tends to focus on IAMs and quantitative results. This section seems more qualitative or "fluffy" in comparison. Again, I'm nit saying that this is not important, but it seems as though the content isn't as quantitatively rigorous as the rest of the chapter.	Rejected. The first level headings have been decided by the IPCC plenary and need to be adhered to by the author team.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3377	6					This section is very interesting. It powerfully argues for taking sustainable development scenarios as benchmark for assessment. The scenarios discussed before look at "climate change mitigation only". Conceptually, 6.6. should not appear as an add-on to the other sections, but rather as a benchmark for overall evaluation. Of course the main challenge is that only very few scenarios so far explicitly address SD. But one could frame the importance of SD scenarios as benchmarks in the introduction of chapter 6.	Noted. The relation of SD to IAMs is discussed in the new version, but otherwise this section is conceptualized as being complementary to the other sections focussing on IAMs linking to / including IAMs where possible.
16252	6					It is important to be more specific about aspects or indicators of sustainable development included in the assessment of the transformation pathways, and which ones are omitted, particularly since several aspects (such as materials (resource depletion) or stocks (standard of living) are not just additions, but may change the nature of the results due to feedbacks or delays). In addition, it might be useful to point out critical gaps in the modeling approach in order to better capture essential links with sustainable development.	Noted. This is in parts covered in Section 6.2.1 (Key characteristics of integrated assessment models) and Chapter 3. Further, also this section is used to link IAMs to SD.
11679	6					As shown in the text, transformation patyways can have a strong impact on broader societal policy objectives relating to sustainable development, such as energy security, food security and water security. When describing sustainable development and climate stabilization, the trade-offs and synergies between two issues should be reviewed as well. For example, Akimoto et al. (forthcoming) conduct a complehensive assessment on these trade-offs and synergies. Reference: K. Akimoto et al., "Consistent assessments of pathways toward sustainable development and climate stabilization", Natural Resource Forum (forthcoming).	Noted.
17921	6					Although the section relates to a large part to SD, SD concepts and SD goals, I have found no cross-reference to Chapter 4 although Chapter 4 is supposed to provide the framing for any SD discussion in the WGIII AR5. For this Section, this is particularly relevant, since SD and the related concepts are not sufficiently explicated. The same applies to the discussion of co-benefits/co-costs and the respective framing in chapters 3 and 4 (which has been nascent in the FOD). Please liaise with the relevant chapters in the cross-cutting meeting to determine a viable labor division and synthesis of results with respect to the co-benefits/co-cost assessment and the relation to SD across chapters.	Accepted. Reference to Ch.4 have been added in the new draft.
3631	6					Delete or massively reduce to save space as overlaps with chapter 4.5.	Accepted. Section 6.4 has been significantly restructured with the new draft.
3632	6					Delete or massively reduce to save space as overlaps with chapters 4.2.1 and 4.5.	Accepted. Section 6.4 has been significantly restructured with the new draft.
3633	6					Delete or massively reduce to save space as overlaps with chapter 4.2.	Accepted. Section 6.4 has been significantly restructured with the new draft.
17925	6					Please consider to incorporate more results from the IAM community (e.g. IMAGE and MESSAGE) - particularly from Chapter 17 of the Global Energy Assessment (CLA Keywan Riahi). This could also prove to be useful for a more in-depth analysis of "regional considerations and differences". At the moment, however, the first four paragraphs of the sub-section rather describe political implications which I would personally place in the policy chapters (e.g. 15.7.1). Please liaise with Navroz Dubash.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9259	6					An additional risk comes from inferred migration patterns, as these may alter societies and energy etc use considerably over decades. I'm not sure how well that could be modelled though. I think this whole chapter provides a wide-ranging and balanced view of things - well done (for this draft anyway!).	Migration patterns due to mitigation policies has not been covered in the mitigation scenario literature, and therefore will not be taken up as a risk trade-off in Chap. 6. It may be discussed qualitatively in other Chapters.
17928	6					In order to facilitate coordination between section 6.7 and the sector chapter discussions on technical risks (see agreements reached in Wellington, p. 36), the classification of different types of risks provided by this section would be very helpful (apart from the framing of environmental side-effects as risks, see my next comment). Please liaise with the relevant chapters in the cross-cutting meeting to determine a viable labor division and synthesis of results with respect to the co-benefits/co-cost assessment and the relation to different types of risks across chapters.	This was done at LAM3 (see Responses to Comments 933 and 953)
17931	6					The topics discussed here are not framed as risks elsewhere in the report but rather as additional policy objectives to which co-benefits/co-costs might accure. While the examples given might be redundant to existing text in other chapters (land-use change in chapter 11, institutional capacityfor and distributional consequences of mitigation policies in chapter 4 and the policy chapters), the section does not discuss the societal risks due to the speed of transformation as advertised in the beginning of the section.	The discussion of potential adverse side-effects has been moved to Section 6.6 Sustainable Development where it will be discussed together with co-benefits. This will include the discussion of societal risks due to the speed of transformation as appropriate. Redundancy has been addressed by coordination with other chapters.
13190	6					This section is currently rather generic, but still rather long. In light of the fact that the authors suggest there will be a number of additions for the SOD, I would suggest cutting down text that overlaps strongly with other chapters (or even sections within this chapter, e.g. some of energy conversion, land use related text).	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
13188	6					Most of this section discusses AR4 approach to sectoral analysis, which doesn't seem necessary, especially to the extent that it's currently done (i.e. over a page) AND taking into account the length of the current draft.	Discussion of comparison between sectoral and integrated studies in AR4 will be shortened.
17475	6					good synthesis of the sectoral analyses but some sub-sections lack comparison with transformation pathways. Specifically "Energy Conversion", "Transport" and "Human Settlements" sub-sections	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14336	6					The chapter should build on more recent comprehensive studies such as Williamson, P., Watson, R.T., Mace, G., Artaxo, P., Bodle, R., Galaz, V., Parker, A., Santillo, D., Vivian, C., Cooper, D., Webbe, J., Cung, A. and E. Woods (2012). Impacts of Climate-Related Geoengineering on Biological Diversity. Part I of: Geoengineering in Relation to the Convention on Biological Diversity: Technical and Regulatory Matters. Secretariat of the Convention on Biological Diversity. Montreal, Technical Series No. 66. The results of the 2004 EIFEX experiment were recently published as: Victor Smetacek, Christine Klaas et al. (2012): Deep carbon export from a Southern Ocean iron-fertilized diatom bloom. Nature doi:10.1038/nature11229. According to a press release, "Unlike the LOHAFEX experiment carried out in 2009, EIFEX has shown that a substantial proportion of carbon from the induced algal bloom sank to the deep sea floor" (see http://www.awi.de/en/news/press_releases/detail/item/current_study_in_the_scientific_journal_nature_researchers_publish_results_of_an_iron_fertilisation/?cHash=1886c469c164291f685e617fe741c704)	Agree that the Williamson et al reference should be included.
9242	6					Please refer 'IPCC expert meeting on geoengineering - meeting report' (2012) for the definition of geo-engineering. It discusses the difference between CDR and mitigation in terms of total storage potential and the impact beyond atmospheric CO2 reduction.	Question: I would assume that IPCC's guidelines suggest that we should cite peer-reviewed literature before reports even IPCC reports. Is that correct?
9245	6					Please mention that cost range of CDR and SRM is uncertain in general.	It's not clear we have room to discuss costs here, if we do we will certainly stress uncertainty.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15438	6					This section fails to convey the speculative nature of SRM -- the current draft exaggerates the state of scientific (and public) understanding, which, in fact, is very poor. We propose that THIS SECTION BE DELETED, OR EDITED to more accurately reflect the state of knowledge. We propose the following edits, which retain much of the information in the current draft, but better convey the speculative nature of SRM, so that the section reads: SRM's possible role in climate policy is shaped by two working assumptions. First, SRM is expected to produce effects soon after deployment, i.e., on a timescale less than a decade (Shepherd et al. 2009); (Keith, 2000; Swart and Marinova, 2010). A further assumption is that SRM could temporarily but imperfectly mask the effects of climate change that arise from the accumulation of long-lived greenhouse gases such as CO2 (though only the reduction in long lived GHGs can reduce the long-run climate risk). Interest in SRM is growing (Shepherd et al. 2009); (Mercer et al., 2011). The notion that SRM could reduce the impacts of anthropogenic climate change dates back to the 1960s (Keith, 2000), but little scientific research has been done. There are now several government-sponsored research programs related to SRM as well as a formal project to systematically compare climate model responses to SRM (Kravitz et al. 2011). Any potential effectiveness of SRM in counteracting anthropogenic climate change is inherently limited by the fact that the radiative forcing produced by SRM techniques (insofar as they exist in theory) is substantially different from the radiative forcing from GHGs (Govindasamy and Caldeira, 2000; Robock et al., 2008). It is therefore impossible for SRM to produce a climate response that precisely compensates for the climate response due to GHGs. Thus while a level of SRM could, in theory, compensate for some of the effect of GHGs on a single climate variable, such as the globally averaged surface temperature, it cannot do so on all variables at once. For example, if SRM is employed in an attempt to halt the increase in globally averaged surface temperature over some period during which GHG concentrations rise, then the global hydrological cycle as measured by average evaporation and precipitation rates will decrease (with potentially significant effects in some regions that will see changes in weather patterns and reduced rainfall). Few studies have attempted to quantitatively evaluate the extent to which SRM could counteract the effects of anthropogenic climate change on a regional basis. The first study to do so concluded that SRM would do a poor job reducing climate damages, and that that damages from SRM might be significant (Robock et al., 2008). More recent studies also assert that (a) SRM cannot accurately reverse GHG driven climate change and that (b) the divergence is larger at regional scales that it is on a global means basis (Ricke et al., 2010), but (c) one study of the potential effectiveness of geoengineering in compensating for temperature or precipitation changes on a regional basis suggests that SRM could compensate for increased GHGs even at a regional level. Using analyses of 22 regions Moreno-Cruz et al concluded that a single (optimal) choice of SRM forcing could reduce the population-weighted mean squared deviation in temperature by 99% and in precipitation by 85% but both cannot be achieved simultaneously (Moreno-Cruz et al., 2012). All modeling studies to date have focused on compensation as measured by a climate variable such as temperature and precipitation; understanding of the effectiveness—or lack thereof—of SRM in reducing climate damages would require understanding of the interactions among the climate variables. Ozone depletion as a consequence of the introduction of geoengineering aerosol into the stratosphere is by far the best studied risk. For sulphate aerosols the primary mechanism of action is that additional aerosol reduces NC	It is not clear exactly what edits are being proposed here

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8960	6					Please avoid the temptation to present geoengineering as a real option for policy makers, since the testing of scenarios is only being done on very limited GCMs and the technology is not just not ready to deploy, but it is dangerous in ways detailed in my work.	Answer: there is no question that SRM is dangerous; however there seems little doubt that some forms could be deployed within a decade or two. This is not a statement that deployment would be wise; rather, it is simply a statement of technical capability. The job of the IPCC is to present a thorough analysis of physically possible options to decision-makers. We are happy to respond to specific suggestions about ways in which the text may overstate the case ability of SRM but this statement is too general to allow a response.
11259	6					Fig. 6.10 is one of the key figures for me, this should come in the executive summary, because it shows the effect of CCS and Bio+CCS availability on the emission profiles. If a figure with the new model results can be provided where one can see from one model the differences of “w CCS”, “w/o CCS” and “w or w/o bio-CCS” this would be extremely interesting	Noted.
11253	6					Concerning the technologies: there is a discrepancy concerning the evaluation of single technologies. In the Executive Summary it is said that “there is no single dominant technology” whereas in the FAQ at the end the importance of Bio-CCS is highlighted. But this importance is not clearly carved out in the chapter.	Accepted. The notion of the importance of a portfolio needs to be made more clearly differentiated from the importance of negative emissions technologies.
9949	6					The executive summary is too long to get the most important points of this chapter. Please make it conciser. Maybe tables and figures can be removed from ES.	Accepted. The ES will be shortened.
8899	6	0				When it comes to economic impacts the study concentrates on outputs GCE and partial equilibrium models that often include only one economic sector. I could not find a section that explains the shortcomings of these model types and how the outcomes are impacted by the theoretical underpinnings of these models nor giving a reasoning why these types of models should be preferred. And there is no explanation of why other model types were excluded. In one place the chapter concentrates on describing the results of only one most recent study and omits all others. This all is hardly justified and makes the chapter look biased and open to critisism. The employment effects of the transition pathways are also ignored.	Noted. We are revising the section on the modeling tools used in this analysis. At the same time, space constraints prevent a thorough treatment of model strengths and weaknesses. Readers are encouraged to read the supporting papers.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6393	6	0				This chapter clearly needs further editing in many ways, with plenty of places stating that the SOD will contain new text or new results of analyses. That's fine, but I think the text could be decreased by about 5% by making the writing more direct. In a lot of places there are extra and unnecessary words. For example, page 20 line 33 could be reduced from "A crucial question with respect to long-term emission reductions is the timing of emission reductions," to "The timing of emission reductions is important for long-term reductions." Or line 8 on page 28, "There is a limited number of studies that..." could be "A limited number of studies..." Or line 34 on page 30, "It is valuable in that it leads to..."could be "it leads to...". Or lne 1, page 31, "A first observation is that there is..." could be "There is..." Or Line 28, page 31, "A further observation is that..." could be, "Further, the costs..." Or page 33, line 31 and 32, "It has been shown that the impact..." could be "The impact..." or page 46, line 6, "The other important role of end use sectors in climate mitigation is to reduce energy demand..." could be, "End use sectors are also important because they reduce..." And so on, throughout the chapter.	Noted. We are working to be more efficient.
6395	6	0				There are references to "the literature" in a number of places without referring to or citing the relevant literature (e.g., p. 40, line 30; p. 60, line 7; p. 60, line 23) Is this an intentional decision? References as such without specific citations seem vague and nonspecific, as if the author(s) are saying, "trust us, the literature says this."	Noted. We will continue to expand our citations of the literature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16945	6	0				It is some time since I look at the “Stabilisation Scenarios” literature and will confine my comments to the following general points. Though it is carefully worded, Chapter 1 comes close to saying that 2 deg.C is now almost impossible: does Chapter 6 analysis support this? Consistency is important. It would be great – albeit difficult – if Chapter 6 could make more connection between the sectoral chapter studies and the global scenarios. Does bottom-up meet top-down? I have suggested in my general AR5 comments that there might be various “meso-level” ways into this; one obvious one would be in relation to the Urbanisation chapter (12), given the enormous influence of urban carbon footprints; and / or the regional chapter (14), which gives a finer-grained understanding of regional trends and possibilities. I’d also like to suggest a third “meso-scale” way of illuminating this challenge. Is it possible to give any indications of how the scenarios might break down in terms of different domain processes, and associated estimates of potentials? Eg. the bottom-up evidence is that First Domain processes (analytically closely tied to the System 1 framing of the FOD Chapter 2) are sizeable in Buildings and maybe Transport efficiency; how much emission reduction might these deliver by mid Century? Does the analysis of “Domain 1” processes shed much light on how much would be taken up in BaU trends, versus requiring policy action? How does this compare to reference scenarios? Similarly, Section 6.5 would be the natural place to integrate the significance of Third Domain potentials – innovation, infrastructure etc - and associated issues (Third Domain effects are only very weakly affected by carbon prices, depending more on strategically motivated investment). Sectoral transformation studies could be other inputs to this. This could be useful as it may help to suggest how much stabilisation scenarios would in practice depend on the different pillars of policy - regulation and 'engagement'; pricing and market structures; or strategic investment for innovation and infrastructure. For clarifications, see Grubb, Hourcade and Neuhoff, Planetary Economics: the Three Domains of Sustainable Development. Chapter 2 (submitted to the Secretariat) clarifies the key distinguishing features of the different domains, Chapter 3 outlines how the main structural components of energy systems seem to relate to each domain, Chapters 4 & 5 detail the evidence and theory around First Domain issues, and Chapters 9-11 explore Third Domain effects; Chapter 11 seeking to offer some integrating insights between meso-sectors and global. The interpretation of information on future scenarios is notoriously subject to “framing” effects. I think it important that IPCC considers the lessons on the importance of these effects and presentation (see eg Mizuno and Klinsky 2012). This chapter in fact seems extremely good in presenting the physical material in transparent accessible forms. However this is much less so for the economic data (from section 6.3 onwards). Once they have established “baselines”, the modelling community almost entirely thinks in terms of changes from these baselines (or NPV loss). Normal people think in terms of absolute changes: are we going to get richer / poorer etc. Somehow it should be made clear that the aggregate economic difference between most mitigation scenarios and baseline is so small as to be hard to see on a graph. Another way of looking at this is the simple exercise of plotting absolute GDP (eg. in 2050) against emission levels: as a “thought experiment” we do this for the entire EMF-22 database in Grubb, Hourcade and Neuhoff (Chapter 11); there is almost no discernible relationship. A final remark: I would guess I am not the only one to find the “negative emission” scenarios with BECCS to be a modelling fantasy which is very hard to relate to the real world. Progress on CCS has been anaemic compared t	Noted. Treatment of the following issues are being revisited in the SOD: linkage to temperature goal such as 2 degrees C; linkages to sectoral chapters; economic results; and negative emissions scenarios.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9819	6	0				In order to enhance the readibility of chapter 6, and, moreover, to reduce its length by at least some of the needed 20 pages by eliminating repetition, we would like to suggest the following reorganization of the arguments: 1. Background relating to AR 4: especially the terms "forecasts" and "scenarios", and their underlying methodological concepts and relevance to AR5, should be differentiated. In order to help the readers to link the two reports (AR4 and AR5), the methodological development of AR5 should be described in somewhat more detail than the current draft does. Thus, the new RCP methodology should be described in this section. 2. Model structure and the underlying theoretical framework for each type of model: in order to enhance readibility the basic model structures could be explained in terms of input variables, assumptions, calculational approach, and linkages between sub-modules. 3. Input assumptions: as the underlying assumptions are crucial for the getting the results, and because different assumptions will lead to different results, the major types of assumptions should be made transparent, and perhaps some numerical values should be given for key variables for different RCP scenarios. Also, the need for different types of input assumptions depending on model structure should be discussed. Following the example of scholarly journals, which often allow the reader to upload supplementary materials, a list of all the major assumptions could be made available for the readers in an electronic appendix. 4. Results: in this section, the broad scope of results could be presented in as neutral and objective way, as possible, with little commentary. For example, don't say things like "consumption is good". 5. Discussion: Separated from the results section, the subjective evaluation of the results could follow, with emphasis on comparing results only between comparable models and scenarios. In order not to oversell results, qualify all results as appropriate. 6. Conclusions for decision/policy makers: Following the results and discussion sections (4 and 5), government and NGO policy makers, and corporate representatives, should be provided conceptual guidance as to how to understand the various kinds of policy implications of the scenarios run thusfar, and how to develop relevant scenarios for their own use. For example, one problem with the current version of Chapter 6 is that there is fairly detailed discussion of some results first, without the proper preparation for the reader, and then some other results are discussed again later, and there is some repetition. Also, consider reducing or eliminating the geo-engineering material which was not really incorporated into existing transformation pathways.	1. Accepted. We will attempt to link to the RCPs. 2. Rejected. We simply do not have sufficient space to go into the details of every model. Readers will be encouraged to look at the underlying literature. In addition, the scenarios database will include informatiion on key drivers such as GDP growth and population growth. 3. Rejected. Readers will be encouraged to look at the underlying literature to find input assumptions. It is beyond the task of this exercise to collect all the input assumptions for every model.In addition, the scenarios database will include informatiion on key drivers such as GDP growth and population growth.4. Noted. 5. Noted. 6. Noted. We continue to refine the storyline for Chapter 6 and will attempt to make it more transparent in the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9820	6	0				1. Many of the results need further qualification stating more transparently under what assumptions the stated conclusion is valid, or not. Two important examples for many conclusions are: "given the limits on the amount of incremental energy efficiency modelled…." , or " given that oil prices were assumed to remain under $100 per barrel forever, the cost results are.....". 2. The types of parameters and their numerical values which are input to the various IA models for different scenarios is, basically, never discussed. This is a major omission. The transparency of scientific research requires that key input assumptions be provided in parallel with all results. This is particularly important for the kinds of scenarios cited in this chapter, because many of the differences between results from model to model are not due to structural differences in the models, but are due to DIFFERENCES IN THE VALUES OF THE SAME INPUT PARAMETERS. 3. Again, the constraints on energy efficiency improvements on the demand side over time input to each IAM must be cited in the text, since the improvement of energy efficiency on the demand side is one of the key technology options to mitigate climate change. Even conceptually, the issue is barely discussed, another omission. 4. The fact that fossil fuel prices are either endogenously computed or exogeneously input to every model is not discussed in any detail. Yet the resulting prices for fossil fuels in any given future year in each scenario might be the single most important factor determining most results. These price assumptions must be presented and discussed in the context of the "peak oil", "peak natural gas", and "peak coal" theories. Fortunately, the peak oil hypothesis is mentioned in the early chapters of this report, but it must be further elaborated relative to its impact on the scenario results presented in Chapter 6. 5. The fact that most if not all the IAMs assume that almost infinite supplies of liquid fuels from "backup" technologies will be available must be discussed and justified, since it is a highly controversial assumption.	1, 2., 3. Rejected. It is not possible to provide all the assumptions associated with every scenario reviewed in this chapter. Readers will be encouraged to explore the scenarios database, which will include information on key drivers such as population and GDP and to review the underlying literature. 4. Noted. All the models include resources for fossil fuels and calculate the associated prices and influence on the nature of the energy system. This information manifests itself in the baseline scenarios and the mitigation scenarios. 5. Noted. The chapter will note the needs for these fuels to meet particular goals. Sectoral chapters will address the technical details of developing these fuel sources.
9821	6	0				The chapter talks about the "costs" of mitigation, by which "net costs" seems to be meant. But no hint is given that depending on the input assumptions there could be "net benefits" for some scenarios in the long run. By thinking and calculating longterm life cycle costs can reveal positive benefits of shortterm costs. For example, this could happen if the long run prices for fossil-fuels were much higher in the reference cases than in the RCP 2.6 cases, where the demand for fossil=fuels would be much lower than in the reference cases. Thus, the wording which seems to imply that the net costs of mitigation would always be positive must be revised and qualified to allow for the possibility of net negative costs. The bottom-line, of course, is that pursuing strong climate mitigation as in the RCP 2.6 type of scenarios might improve other aspects of the economy and consumption, if, indeed consumption is always good.	Accepted. We will mention the issue of negative costs. At the same time, the literature we are reviewing overwhelmingly indicates that there will be positive costs.
9822	6	0				As hinted at in the above comments, the whole scenario "infeasibility" discussion, which repeats itself too often, forgets to mention that one major cause of apparent infeasibility for some scenarios for some models is probably the overly limited level of end-use energy efficiency improvements. IAMs and their outputs aside, the RCP2.6 greenhouse gas trajectories required are not infeasible given existing energy efficiency technologies and given renewable supply technologies, even with CCS and nuclear power. This must be made clear to the reader. Again, this is another situation where the results are not properly qualified based on model limitations or input assumptions made. Moreover the reader should be aware, that assumption can be changed by decision-makers.	Noted. We are refining the discussion of situations in which models are unable to produce particular scenarios.
9823	6	0				For decision makers a conversion of the RCPs to degrees Celsius could be valuable: Rogelj, J; Meinshausen, M.; Knutti, R. (2012) Global warming under old and new scenarios using IPCC climate sensitivity range estimates. In: Nature Climate Change, DOI: 10.1038/NCLIMATE1385)	Accepted. A section is being added that will provide a crosswalk between concentration and temperature goals.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9404	6	0				When discussing transformation/transition pathways, it is also important to discuss and focus on shor-term/mid-term targets for reducing GHG emissions as well as long-term GHG emission projections. These following papers can provide useful information on GHG emissions by region (e.g. Japan, China, India, All Asia, USA, EU27, Russia, Annex I , Non Annex I and world) and by technological mitigation cost (e.g. 0, 25, 50, 75, 100, 125, 150, 175, 200 US$/tCO2) in the year 2020 and 2030, based on bottom-up analyses. Hanaoka, et al, 2012 also provides technological mitigation potentials by region, by cost and by sector in the year 2020 and 2030. These discussions may be fit into section 6.4, but these papers are missing in this chapter. Dr. Hanaoka can help providing data for this chapter. 1) Hanaoka, T., Kainuma, M. (2012) Low-Carbon Transitions in the World Regions: Comparisons of Technological Mitigation Potentials and Costs in 2020 and 2030 by bottom-up analyses. Sustainability Science, 7(2):117-137, DOI:10.1007/s11625-012-0172-6 2) Akashi, O., Hanaoka, T. (2012) Technological feasibility and costs of achieving a 50 % reduction of global GHG emissions by 2050: Mid- and long-term perspectives. Sustainability Science, 7(2):139-156, DOI: 10.1007/s11625-012-0166-4 3) Wagner, F., et al (2012) Sectoral marginal abatement cost curves: implications for mitigation pledges and air pollution co-benefits for Annex I countries, Sustainability Science, 7(2):169-184. DOI:10.1007/s11625-012-0167-3 4) Akimoto, K. et al (2012) Comparison of marginal abatement cost curves for 2020 and 2030: longer perspectives for effective global GHG emission reductions, Sustainability Science, 7(2):157-168, DOI:10.1007/s11625-012-0165-5	Noted. We appreciate the reviewer offering additional literature for citation. We will use if appropriate.
9405	6	0				Main discussions in this chapter are features of CO2 emissions that are of course important. However, in discussions on transformation pathways, it is also important to take into account non-CO2 GHG emissions, not only Kyoto gases but also air pollutants such as BC, OC (short-lived gases) and Montreal gases such as CFCs and HCFCs (long-lived gases). As for CFCs and HCFCs, these are long-lived gases with very high global warming potentials that the policy makers were aware of and there will be still large amount of emissions in CO2 equivalent in the next 10 -20 years which are difficult to be reduced even regulated under the Montreal Protocol. Thus, it is also important to be mentioned when discussing the short-/mid-term emissions pathways. The following papers are just examples which provide new findings after the IPCC AR4, and authors can review various other papers and reports by UNEP, WMO etc. 1) Velders, Guus J.M., Stephen O. Andersen, John S. Daniel, David W. Fahey, and Mack McFarland. 2007. The importance of the Montreal Protocol in protecting climate, PNAS 104(12): 4814–4819. 2) Velders, Guus J.M., David W. Fahey, John S. Daniel, Mack McFarland, and Stephen O. Andersen. 2009. The large contribution of projected HFC emissions to future climate forcing. PNAS 106(27):10949–10954. 3)Wan, Dan, Jianhua Xu, Jianbo Zhang, Xuanchang Tong, and Jianxin Hu. 2009. Historical and projected emissions of major halocarbons in China. Atmospheric Environment 43: 5822–5829	Accepted. We will include more discussion on short-lived species in the SOD.
16909	6	0				There needs to be more synergy and consideration of cross-cutting issues between the chapters. For example, chapter 4 has a constructive discussion about the need to distinguish subsistance emissions, development emissions and luxury emissions. Yet in the all scenario studies in chapter 6, there is no indication that any one of the scenario projections make the distinction between survival emissions vs. Luxury emissions.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13147	6	0				It's a bit unfortunately that the dataset, and therefore most of the analysis, was not finalized for the FOD. There are findings across the chapter that would in their current form require some clarifications/explanations, but it doesn't seem useful to ask for such at the moment, when there is always a caveat close by, suggesting that the analysis so far shouldn't be taken too seriously.	Noted.
13156	6	0				More references should be added, throughout the chapter, to results that are currently referenced using a project name (e.g. EMFXX, RECIPE etc) only.	Accepted. We will continue to add references as the process proceeds.
13166	6	0				The scenario ensemble used for the chapter appears to somewhat dominate the discussion, especially in some sections. This is also demonstrated by the fact that in certain sections a proper literature review is almost completely missing and the references mostly focus on the few large studies that also feed in to the database (and in which a number of the authors have been involved). While this is a useful approach in terms of giving detailed information about a large set of scenarios, it also creates an impression that nothing else has been done within this field during the past years. I would recommend that the database related results are balanced with literature reviews, so that it's ensured that the large model comparison studies are not represented as the only forum in which research has been done (and if it turns out there actually are few relevant studies beyond the comparison projects, so be it, at least it can then be stated as a justification for the approach taken). Also, small, individual studies may well bring in additional, alternative approaches that could enrich the scenario space (i.e. relying on model comparison studies (e.g. 10 models run 5 different scenarios) provides useful information about differences across the models, but less information about the heterogenuity of the possible scenario space (compared to 50 different scenarios, each run by 1 model alone)).	Accepted. We will continue to add references to a broader swath of literature as the process proceeds.
11242	6	0				The chapter is full of very interesting details, but it has no clear message. Is there a story of decarbonization? Is every model telling its own story?	Noted.We are continuing to refine the story of the chapter heading into the SOD.
11245	6	0				In the headings of the sections 6.3.4. and 6.3.4.2 the “idealzed context” is mentioned, but there is no chapter that is called “non-idealized context”. On my opinion these two viewpoints “idealized vs. non-idealized” have to be contrasted. It is somewhere hidden in the text, but it is not told and sold as a story. I think that this could give a framing for the whole chapter.	Accepted. We will explain the distinction more clearly in the SOD.
11247	6	0				I am missing something like “no regret options”. This could be an interesting framing, e.g. no matter which delay we face, no matter which stabilization target should be achieved, technology X is always important and is required at a deployment level of Y.	Noted. We have not decided whether to use the notion of "no-regrets" options as part of the framing of the story in the chapter. We are considering alternnative framings for explaining the sorts of actions that seem to be found across scenarios.
11248	6	0				Will there anywhere in the report be a translation of the RCP forcings to temperatures? If it is in WG1, it should be repeated here just for information.	Accepted. Yup. We're working on that for the SOD.
11249	6	0				There is no reference to the 2Deg target and also the discussion of 1.5 vs. 2Deg is missing. But this has been requested by the UNFCCC. It would be important to come up with some messages on this issue.	Accepted. While the chapter will focus on stabiliization of greenhouse gases, consistent with Article 2, we will include a linkage between those goals and associated RF pathways and temperature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11251	6	0				There is a clear bias in the whole chapter to overstate the importance of nuclear. Often it is mentioned "technologies such as CCS and nuclear". There is no single indication that nuclear is as important as CCS from this chapter. Quite the contrary, Figs e.g. 6.29, 6.30 etc clearly show that CCS has a big effect on the cost, but not nuclear. It is quite striking that in the whole chapter there is nearly no reference to the individual technology "nuclear" but it is only mentioned in one breath with coal+CCS. A standard sentence in chp. 6 is "….particular technologies such as nuclear power or fossil energy with CCS". But your cost figures clearly indicate that there is a huge difference between the importance of CCS and nuclear. This is not stressed at all.	Noted. We will be adjusting the text of the chapter and will continue to refine the discussion of different technologies.
11252	6	0				Something similar to Figure 13.3. (FOD, chp. 13) would be extremely useful in the Executive Summary. On the x-axis should be e.g. the technology setting is given, indicating the interplay between technology choice and emissions. You have the figure for the costs, but the according figure for emissions would be helpful.	Noted.
11254	6	0				I miss a discussion on energy efficiency. This is one of the most important options, already in the basline in some of the models (and coming as a free lunch) but it is not discussed as an important option.	Noted. We have a discussion on end use efficiency. We expect to refine that discussion in the SOD. In addition, a more refined discussion in the section on linkages to sectoral analysis will help to bring out the role of end use sectors.
11262	6	0				There should be some assumptions given on the CCS use. What do the models assume when CCS is available? What is the assumed storage capacity? Leakage rate etc. The same for biomass? Is there a limit on biomass use? This is important for interpreting the results.	Rejected. This synthesis does not have the space to consider all the assumptions of all the scenarios used in the analysis. Readers will be encouraged to look at the underlying literature.
11267	6	0				Are the carbon prices shown? If not – why?	Noted. We have included carbon prices in one parrt of the chapter only. We are considering whether to include them in other parts as well or whether to remove them entirely given space constraints. The value of carbon prices as an indicator of cost can often be overused.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11015	6	0				The Chapter explores the implications of stabilizing atmospheric GHG concentrations. An effort to implement such a goal, were it ever to be attempted, would constitute global social engineering on a completely unprecedented scale. There is little evidence that such an effort will be made. Indeed, on page 26, the Chapter notes "…no cost-benefit study finds an optimal level of mitigation that stabilizes atmospheric concentrations within the modeling period." While one should be mindful of the optimization models' limitations, surely the point represents a significant caveat to the analysis. Giving the point more prominence in the Chapter would seem, therefore, to be appropriate. The caveat seems especially apposite in that IAMs ignore institutional constraints on the choice and implementation of mitigation policies. Based on current experience with global mitigation efforts, institutional constraints deter many countries from acting on mitigation and induce others to use policies with poor cost-effectiveness. The inference seems to support the conjecture that the scenarios described in the Chapter would diminish global welfare. Many governments, however, have at least formally embraced some version of stabilization goal. Thus, a discussion of its implications is may be useful. In this regard, the chapter makes many welcome points. It is particularly welcome that the Chapter notes that a capacity for solar radiation management (SRM) might be an important tool in coping with low probability high impact events. Previous IPCC reports have slighted the potentia importance of SRM, and in this regard, Chapter 6 makes a valuable new contribution. The Chapter discusses the subject in a fair and balanced manner.	(1) The discussion of cost-benefit analysis is being moved to a distinct box. We do not intend to treat such analysis at length in this chapter and instead will focus on stabilization, consistent with Article 2 of the UNFCCC. (2) We agree with the reviewer about institutional constraints. However, the purpose of this chapter is to highlight what would need to happen to meet particular goals, with later chapters assessing how hard or easy it might be to make that happen given institutional and other constraints. (3) Comments on SRM noted.
16349	6	0				Please consider using the following paper in your assessment: The world at a crossroads: Financial scenarios for sustainability Jofre Carnicer and Josep Peñuelas Energy Policy 48, 2012p 611-617	Noted.
16351	6	0				The structure of this chapter may benefit from a revision: The current section 6.3 is very long and includes a lot of different issues. It discusses costs of mitigation before sustainable development and transformation pathways (6.6.) - if this remains in that order, efforts are needed to avoid treating costs without sufficiently taking into account their context (baselines, including level of sustainable development, may strongly influence costs).	Noted. Unfortunately, the outline handed down by the plenary places an enormous amount of material in one section (6.3) with an absurdly long title. At this point, the strategy is simply to split that up into its constituent pieces within 6.3.
15717	6	0				A general point. The RCPs assume a considerable phasing out of aerosol emissions by the end of the century. These are 'best guesses' but the radiative forcing of aerosols contain large uncertainties. Particularly in the high end RCPs, that could lead to an under-estimation of the aerosol cooling from SOx, NOx , sulphates and nitrates and hence an overestimation of the increase in temperature by 2100, which would make IPCC an easy target for critism. Of course there are uncertainties both ways (black carbon and troposperic ozone have a positive forcing). However, as the RCPs are the source for climate model projections I recommend to take this issue up with WG I and see if such critisim can be prevented (nb aerosols are addressed in 8.2.2 but not in the context of RCPs)	Noted. Additional dicussion of aerosols and their relationship to transformation pathways will be included in Chapter 6.
18624	6	0				Results are preliminary and the chapter is far from complete	Noted.
18627	6	0				A failure to include land use change emissions into the mitigationregime could dramatically increase the difficulty of meeting long term goals, and it could potentially lead to dramatic changes in the global land surface. The ability to store CO2 using bioenergy with CCS or other CDR technologies facilitates overshoot pathways….	Noted. Both of these points are in the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
18628	6	0				The pathway discussion lacks a clear subject, a pathway to whom? It also suffers from a more general knowledge gap; we don't know what innovation and tech will bring.	(1) Noted. The pathways are leading to stabilization of GHG concentrations or RF. (2) Noted. The chapter addresses the importance of innovation for mitigation goals.
18629	6	0				I lack a more "philosophical" discussion on how to approach the future, how do we get from here to there. The approach is rather mechanical. At the same time we know that we don't have or can gain full knowledge. Even more important, choices made will have a crucial effect on how different options develop (keeping everything open isn't hardly a real option.	Noted. The framing chapters are intended to address these broader issues.
18630	6	0				Our knowledge about the future is and will be limited. Different approaches possible. Predict, "calculate" or choose directions. It is unclear what the ambition really is but for time perspectives over decades it can't be correct to look into parts and assume that all other things are equal. Wouldn't it be more interesting to compare different directions and where they will lead in terms of capabilities that will develop?	Noted. The chapter includes a discussion of the relationship between sort-term actions and long-term goals. This discussion will be refined in the SOD.
18631	6	0				Decarbonisation is a learning process (for society), different platforms should be compared.	Noted.
18636	6	0				Transformation to atmospheric stabilization is best understood as a process of sequential decision-making and learing.	Noted. The chapter includes a discussion of sequential decision-making. This discussion will be refined in the SOD.
18637	6	0				Near-term emissions need not necessarily be in the optimal range for a long-term goal to be met.	Noted. The chapter makes this point.
18638	6	0				While it is clear that some mitigtion effort in the near-term is crucial to preserve the option of achieving low stabilization targets, whether these targets are met in the long-run depends to a greater extent on the potential for deep emissions reductions several decades from now. Thus efforts to begin the transformtion toward stabilization must also be directed toward developing the technologies and institutions that will enable deep future emissions cuts rather than exclusively on meeting particular near-term targets…. The benefit of beginning to create and improve technologies today and to develop institutional capacity is that it creates opportunities to make early and mid-course corrections.	Noted. The chapter includes a discussion of this issue.
18639	6	0				Flexible market-based policies with maximal sectoral and geographic coverage are most likely to deliver emissions reductions at the lowest economic cost. Although the added cost of inefficient policies in the near-term may be smaller than in the long-term when mitigation requirements wil be much larger, their implementation now may lead to "institutional lock-in" if policy reform proves difficult. Thus a near-term focus on developing institutions such as domestic and international emissions trading markets (as in the European Union's ETS), as well as political structures to manage the large capital flows associated with carbon pricing, could provie substantial dividends in he coming decades when mitigation efforts reach their ful proportions.	Noted. The discussion policy regimes and institutional lock-in is left to the policy chapters. This chapter makes clear that costs are lowest under idealized, price-based policies.
9029	6	0				1. The Chapter is a work in progress. Its approach of classifying the results of different models into six categories is useful for summarizing the variety of approaches and results. 2. The draft has clearly marked loose ends, where it points to simulations that are not yet completed. 3. The chapter should be commended for surwewing the literature on sustainable development (6.6). Unlike standard economics approaches this approach recognizes the developmental gaps (in incomes, employment, and technology) that characterizes developing countries. In contrast, Chapter 3 which surveys economic analysis frameworks leaves to Chapter 4 the consideration of sustainable development.	1. Noted. 2. Noted. 3. Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9030	6	0				Chapter authors mights consider being more accurate and circumspect about statements to the effect that developing countries will bear greater mitigation costs. Assessment models do reflect the fact that the future mitigation potential will be greater in developing countries because BAU assumptions assume that development will occur (despite the variety of assumptions on per capita income, energy intensity, China and India prospects - see lines 9-10 p. 18). Mitigation potential is different from the bearing of mitigation costs. There are two things that must be taken into account. First, the Framework Convention provides that the incremental costs of mitigation wil be provided by the developed countries. Secondly, development might not occur for many reasons including perhaps due to the fact that developing countries are unable to achieve the presumed investment, trade, growth regimes because of the climate regime or the international economic regime in general.	Noted. The chapter will make clear that a requirement for the assertion of higher costs in the developing regions is continued growth in those regions. In addition, a section on burden-sharing is being added to the chapter.
9033	6	0				A fundamental weakness of the chapter is in presenting scenarios is that it does not start with a differentiation of mitigation potential and emission flows in developing countries. Achieving the mitigation potential in developing countries will require the availability of means of implementation.	Noted.
9034	6	0				A fundamental weakness of the chapter is in presenting scenarios is that it does not start with a differentiation of mitigation potential and emission flows in developing countries. Achieving the mitigation potential in developing countries will require the availability of means of implementation.	Noted.
9035	6	0				An important fundamental inaccuracy in the chapter is that it estimates cost of climate policies but these costs are not netted against the economic costs of climate change itself. The scenarios appear not to incorporate the net costs of climate change.	Rejected. This chapter is not addressing the benefits of mitigation. The chapter will recognize the lack of inclusion of climate impacts into stabilization scenarios as a major research gap.
9036	6	0				The scenarios presented in the Chapter include the possible impact of incomplete or late participation in climate change policies. However, these simulations appear to only have developing countries as lagging participants. There should be scenarios in which the impact of incomplete, delayed or non-particiption by developed countries, notably the United States, are reported.	Noted. This chapter is synthesizing whatever literature is available. The available literature focuses on full global delays or delays by developing regions. The basic insights can be extrapolated to scenarios in which the developed countries delay and the developing countries take immediate action.
9037	6	0				The scenarios chosen for presentation in the Chapter are incomplete and do not reflect or straddle the full range of possibilities, including the potential for a serious implementation of sustainable development framework. Among the scenarios that appear to be “missing” are the following: (1) (1) a scenario based on improving distribution of income through time, and its impact through changing lifestyle, reduced consumption in the rich countries; (2) a scenario showing the impact of no or inadequate climate financing for developing countries against a scenario of adequate financing for climate change ; (3) the current scenarios assume that carbon taxes are the only source of climate finance; there should simulations which assume other sources for financing for climate change.	Noted. Such scenarios will be included if they can be found in the peer-revieiwed literature.
9038	6	0				Many, if not the overwhelming majority of the scenarios, assume negative emissions in the out years. What are the implications of these global numbers for burden sharing across countries? What are the implicit assumptions or implications of these scenarios about financing and technology transfer of negative emissions scenarios?	Noted. This chapter will highlight the needs for technologies. A later chapter will explore financing issues.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8604	6	0				1. Many of the results need further qualification stating more transparently under what assumptions the stated conclusion is valid, or not. Two important examples for many conclusions are: "given the limits on the amount of incremental energy efficiency modelled…." , or " given that oil prices were assumed to remain under $100 per barrel forever, the cost results are.....". 2. The types of parameters and their numerical values which are input to the various IA models for different scenarios is, basically, never discussed. This is a major omission. The transparency of scientific research requires that key input assumptions be provided in parallel with all results. This is particularly important for the kinds of scenarios cited in this chapter, because many of the differences between results from model to model are not due to structural differences in the models, but are due to DIFFERENCES IN THE VALUES OF THE SAME INPUT PARAMETERS. 3. Again, the constraints on energy efficiency improvements on the demand side over time input to each IAM must be cited in the text, since the improvement of energy efficiency on the demand side is one of the key technology options to mitigate climate change. Even conceptually, the issue is barely discussed, another omission. 4. The fact that fossil fuel prices are either endogenously computed or exogeneously input to every model is not discussed in any detail. Yet the resulting prices for fossil fuels in any given future year in each scenario might be the single most important factor determining most results. These price assumptions must be presented and discussed in the context of the "peak oil", "peak natural gas", and "peak coal" theories. Fortunately, the peak oil hypothesis is mentioned in the early chapters of this report, but it must be further elaborated relative to its impact on the scenario results presented in Chapter 6. 5. The fact that most if not all the IAMs assume that almost infinite supplies of liquid fuels from "backup" technologies will be available must be discussed and justified, since it is a highly controversial assumption.	Please see the response to comment 9820, which appears to be a duplicate of this comment, despite being submitted by another reviewer.
8605	6	0				The labels on each figure and table need to be made more clearly understandable in many cases.	Accepted.
8606	6	0				The chapter on talks about the "costs" of mitigation, by which "net costs" seems to be meant. But no hint is given that depending on the input assumptions there could be "net benefits" for some scenarios in the long run. For example, this could happen i	Noted. It is made clear in the section on tools of analysis that input assumptions and model structure can have an important impact on costs and other results.
8607	6	0				The discussion of the net cost results from each model run for scenarios is fairly weak because while there is some discussion of the aggregate type of costs produced by different types of models (e.g. GDP vs. other aggregates), the discussion does not state what function kinds of costs are included in each model, e.g. investment costs, operating costs, O&M costs, capital additions. Similarly, the reader is not told if, for the energy system, the incremental costs of energy efficiency are included (I think not), or transaction costs, or infra-structure costs, etc.	Noted. The discussion of cost metrics and their pros and cons is being moved to the metrics annex. To the degree possible, the chapter will highlight the use of different metrics.
8608	6	0				The chapter on talks about the "costs" of mitigation, by which "net costs" seems to be meant. But no hint is given that depending on the input assumptions there could be "net benefits" for some scenarios in the long run. For example, this could happen if the long run prices for fossil-fuels were much higher in the reference cases than in the RCP 2.6 cases, where the demand for fossil=fuels would be much lower than in the reference cases. Thus, the wording which seems to imply that the net costs of mitigation would always be positive must be revised and qualified to allow for the possibility of net negative costs. The bottom-line, of course, is that pursuing strong climate mitigation as in the RCP 2.6 type of scenarios might improve other aspects of the economy and consumption, if, indeed consumption is always good.	Please see the response to comment 9821, which appears to be a duplicate of this comment, despite being submitted by another reviewer.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8609	6	0				As hinted at in the above comments, the whole scenario "infeasibility" discussion, which repeats itself too often, forgets to mention that one major cause of apparent infeasibility for some scenarios for some models is probably the overly limited level of end-use energy efficiency improvements. IAMs and their outputs aside, the RCP2.6 greenhouse gas trajectories required are not infeasible given existing energy efficiency technologies and given renewable supply technologies, even with CCS and nuclear power. This must be made clear to the reader. Again, this is another situation where the results are not properly qualified based on model limitations or input assumptions made.	Please see the response to comment 9822, which appears to be a duplicate of this comment, despite being submitted by another reviewer.
8610	6	0				The chapter could be organized more logically. I suggest the following order: introduction with discussion of scenarios vs. forecasts; model structure; input assumptions; neutral presentation of results; discussion of results and overall conclusions regarding transformation pathways; policy issues that arise and policy implications of the results.	Noted.
5427	6	0				The Chapter focusses on scenarios and models created by or for vested interests, and proceeds to make the usual error of looking at the area with most dense scenario forecasts. This is false, because the data employed are all in this "dont rock the boat" category. The more extreme scenarios are few because of the way scenario data were collected, but may well hold the most interesting suggestions for policy implementation.	Noted. We might note to the reviewer that the scenarios required to meet 450 ppmv CO2 appear to include some rather dramatic changes from historical trends.
3145	6	0				chapter 1 needs an iconic figure from chapter 6 that would help us illustrate the cost (and infeasibility) of some emission pathways and goals. Can the Chapter 6 team advise us on that—we need to replace figure 1.8, which is an old EMF chart and does not illustrate the key point.) ONe option would be to move figure 6.11 plus a figure that shows infeasibilty/costs into chapter 1. TSU PLEASE HELP ADVISE. BECCS plays a huge role in the IAMs that can meet goals like 2 degrees. Given that, why not use BECCS as a case study/box in chapter 3 since that would help tie together the issues discussed there with the large role that is assumed for BECCS in some scenarios. As a general matter, this chapter has lots of terrific material and almost no connection to the rest of WG3. Discussions of financial transfers have no bearing on chapter 16 (which is on finance). (Chapter 16, itself, is a mess.) Discussion of LUCF seems disconnected from the land use chapter and from WG1. The TSU needs to help figure out which connections are most important, and if the transition work discussed in this chapter is pivotal (it is really mainly EMF work) then the TSU might want to help move a few figures from here to other chapters to tie the WG3 together to a greater extent. (one of the notable exceptions to the above comment is chapter 9.9.1, which uses info from chapter 6 to discuss the size of the mitigation challenge in the buildings sector.)	1. Noted. We're working on it. 2. That is an issue for Chapter 3 to consider, but it seems like a good idea. 3. Agreed on connections. Efforts will be made to brinng in more connections in the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
18459	6	0				This is a fine draft by an excellent author team. My concern is that, by very largely limiting the chapter's scope to issues that have been incorporated in Integrated Assessment Modeling (IAM) scenarios, they open themselves u to criticisms that there are other knowledge bases related to transition pathways that are being overlooked. One example would be literatures related to the HOW of transition pathways, as distinguished from the WHAT: i.e., institutional implications and requirements. The chapter has a bit of this (e.g., pp. 53-54, 64, and sections 6.7 and 6.8.3), but these discussions generally just note the issues but then back off from discussing them because they are not embedded in IAM scenarios. I would suggest adding a couple of experts on institutional aspects of transition pathways, at least as contributing authors, in order to fill this kind of (possible?) gap. Regarding chapter length, it is clear that the problem is section 6.3, which runs nearly 40 pages. A good deal of this might be summarized from the available literature, referring readers to the original reports if they want more detail.	Rejected. We agree with the reviewer about the need to think about the challenges in making the sorts of transformations diiscussed in this chapter. But those discussions are not intended in this chapter, but are rather the purview of later chapters, such as Chapter 15 on national and sub?national policies and institutions and Chapter 13 on international policies policies and institutions. We will, however, provide more linkages to these other chapters.
6504	6	0				Both US$ and $ should be unified. Because they are used thoroughout Chapter 6.	Editorial.
18993	6	0				Main comment: The chapter needs to clearly communicate the feasibility of the 2° target and outline possible pathways to reach it and the costs and consequences associated with it. Likewise the chapter also needs to cover "enhancing mitigation options" (UNFCCC), i.e. mitigation targets more stringent than 2°, also including costs and consequences. This will require a clearer discussion about peaking, not-to-exceed and overshoots. Also trade-offs between different pathways should be discussed.	Noted. The chapter will now do a better job of linking the RF stabilization scenarios to temperature goals.
18994	6	0				Main comment: The chapter needs to focus on the exploration of different transformation pathways and their institutional requirements. Carving out different characteristic classes of pathways will require a deeper analysis and clustering of the scenarios.	Noted.
18995	6	0				Main comment: The chapter needs to communicate clearer that different scenarios/pathways are not representing a wide range of statistical uncertainty but are rather demonstrating different technological, energy efficiency, etc. options/strategies and structural uncertainty (represented through differences in models and their assumptions).	Noted. Text to this effect was included in the FOD, but it will be refined for the SOD.
18996	6	0				Main comment: The chapter should aim to inform policy makers about no-regret options, crucial technologies and robust strategies.	Noted. The chapter will continue to be refined to bring out the major insights about actions needed to meet various long-term goals.
18997	6	0				Main comment: The chapter mentions in the ES that dramatic changes are needed, but does not convey in the chapter why the presented options/pathways are dramatic and what their implications are. This has to be communicated in a manner understandable for policy makers.	Noted. More effort will be made to communicate the scale of changes required to meet different goals.
18998	6	0				The chapter should give stabilization targets (in degrees) rather than (or in addition to) forcing targets	Rejected. There is too much uncertainty in the relationship betwee RF and temperature to use temperature as the defining characteristic of different pathways. Instead, the chapter will now include a section that describes how to interpret RF scenarios in the context of temperature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
18999	6	0				The discussion of technologies required for certain pathways also needs to cover technology risks. Possible these issues could be covered by the respective sectoral chapters and referenced from Chapter 6.	Noted. Risks will be covered in the sectoral chapters and summarized in Chapter 6.
19000	6	0				The possible renaissance of coal should in our view be centrally discussed in the chapter.	Rejected. A possible "renaissance of coal" as well as a range of other key drivers are embodied in the baseline scenarios section. With limited space, this section will address the overarching insights from that literature rather than specific drivers such as the "renaissance of coal".
19001	6	0				The chapter should cover the historic context, i.e. transformation pathways in AR4 and SRREN and focus on what is new in recent scenarios (e.g. BECCS)	Accepted. The chapter will now be framed more in the context of what is new since AR4.
19002	6	0				The chapter should highlight the role of energy efficiency and how it is (implicitly) covered in IAMs.	Accepted. More effort will be made to clarify the role of end uses in mitigation.
19003	6	0				The chapter should further pursue the process of linking scenario data (“top-down”) with sectoral data (“bottom-up”).	Accepted. The SOD will include a more extensive treatment of the linkage between top-down and bottom-up analysis through the discussion of the linkages to sectoral analyses.
19004	6	0				Figures on deployment of technologies are needed, as this is of great interest to policy makers. Consider Chapter 6 summarizing this from the sectoral chapters.	Noted. Figures on total deployment will be found in this chapter, but deploymetn of individual supply technologies will be more extensively covered in Chapter 7.
9031	6	0				The bases and the implications of model infeasibility is one of areas where the chapter is incomplete in terms of analysis and simulations. There are references to sections 6.2.5, 6.2.7 that are not in the first draft. One of the key issues the drafters seem to require a clear agreement on is the treatment of technologies that make possible negative emissions. An analytical approach and clearer view of the role of these technologies is important because of its potential role in creasing development space and the greater responsibility that developed countries could bear in the use of these technologies.	Rejected--space constraints
9032	6	0				The chapter confines its survey to integrated assessment models.	Noted
18635	6	0				Section 6.4 contains an interesting reasoning on how to integrate different time perspectives (relates to some of the issues that I have raised above).	OK.
9192	6	1				ch6 needs coordination with "service industry" sectin of ch9 (building)	Rejected. This is beyond the scope of Chapter 6.
9193	6	1				the risk management perspective of mitigation, adaptation, and geoengineering should be described here with coordination with ch 1& 2	Accepted in part. Chapter 6 will not be addressing adaptation. However, it will include discussion of how to think about SRM in a risk management framework.
9191	6	1				footnote should be added that says the frequency (number of scenarios) is not probability.	Footnote has been added

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14030	6	1		99		As the theme of transformation is receiving increased attention both in scientific and policy discourses, we see that the concept takes on multiple meanings and uses. In the SREX report of the IPCC (2012) transformation is defined as “The altering of fundamental attributes of a system (including value systems; regulatory, legislative, or bureaucratic regimes; financial institutions; and technological or biological systems.” And it chapter 8 of the SREX report the personal, cultural, institutional and systems levels changes are discussed in more detail. Ch 6 looks at transformation of the energy system, both on production and demand. It discusses what stabilization levels are possible, how we can get to these stabilization levels and how decisions today will influence future stabilization paths. In this chapter, transformational change is limited to the goal of stabilization of greenhouse gas concentrations, and it is very much focused on the energy systems, using large-scale integrated models to capture the interaction between different processes and systems. Even though the chapter points to the larger social context, it does not discuss what personal, cultural, institutional, and systems transformations are needed in the face of climate change. It does not say anything about what a well-adapted global society would look like, or the transformations that are needed to address vulnerability or adapt to inevitable impacts associated with different stabilization pathways. In fact it actually sees little role of adaptation in a transformative pathway. There is a broad literature that argue that climate change responses require far more than technical solutions, they also call for transformations in the systems and structures, at different scales, and in different contexts and settings, that currently promote undesirable outcomes. These may include transformation in energy systems as covered in this chapter, but it also call for transformation in agricultural systems, financial systems, governance and development paradigms, power and gender relations, production and consumption patterns and lifestyles, knowledge production systems, or values and worlds views. My worry is that the reader of AR5 will be left confused of what transformation actually means for society and the environment, given its multiple meanings and uses in this report. Chapter 6 is about stabilization pathways, and I’m curious why this term is not used instead of transformational pathways. This would be an important clarification to be made upfront together with a definition of transformation.	Accepted. The SOD will include more references to later chapters that address these elements of the transformation. Chapter 6 will focus most heavily on the nature of the physical transformations.
13560	6	1				There seems a significant overlap between chapter 5 and 6 in terms of driver analysis, trends, emission reduction options….	Noted.
10399	6	1		99		The parameters about the discount rates are not so clear. And functions are lacked for the technology progress. As the greater and greater role the technology plays in the abatment of carbon and the new technology achieved these years, it cannot be ignored for the medium and long run models. Maybe there is a few papars talked about this issue, but papars on this issue indeed exist,such as Zwaan,Gerlagh,et al(2002),Buonanno, carraro, Galeotti(2003),Wu, Zhu, Wang(2012),Wang,li, Wu(2010).	Noted. More discussion of discounting assumptions are needed. Readers will be directed to Chapter 3 for a more extensive discussion of discounting issues.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5853	6	1	1	99	40	At several places in the text you refer to "carbon-free" or "zero-carbon electricity sources". These do not exist. Nuclear power installations cause emissions during construction, maintenance, operation, intermediate and / or final waste storage, fossil energy with CCS just reduces efficiency to capture and store C which "is still there" (and this source also has emissions from construction, operation and maintenance), and "renewables", especially biomass-based, of course also have C emissions! Low or "zero" emissions during the "electricity generating phase" have to be related to the life-cycle emissions of the "electricity generating device" to get the complete assessment. Speaking of "carbon-free sources" just promotes "emit now, save later", or in case of nuclear waste: "emit now, save a little later, emit for generations to come" and thus I strongly suggest to avoid the use of terms like "zero carbon" or "carbon-free".	Noted. It may be wise to move to a nomenclature on low-carbon technologies.
14036	6	10				Would it also be appropriate to say that the model only covers limited criterias for transformation, as non-market factors are not considered. Raskin and collegues (2002) for example find that critical events/disasters define what decisions are made and what pathway society gets on (Raskin, P. et al. 2002. Great Transition: The Promise and Lure of the Times Ahead. SEI,Tellus Institute).	Noted.
13129	6	10	13	10	15	This contradicts what was said previously about feasibility being subjective (beyond the biogeophysical constraints). If it's truly subjective, how could this chapter provide information about near term actions that prevent certain long term goals (except for the near term actions that break the long term goals already in the near term, of course)? And to follow the logic of the previous paragraph, does this chapter discuss perceptions of feasibility, rather than actual feasibility?	Noted. The manner in which near-term actions interact with options to meet long-term goals will be revisite in the SOD.
4193	6	10	44	11	10	Tradeoff between "detailed formulation" and "intutive understanding" or "flexibility to represent uncertainties" would be also touched upon.	Noted
16689	6	10	7		15	Very important point -- suggest this be moved forward in the document.	Noted.
4192	6	10	32			Progress of model development since TAR or AR4 should be touched upon. In my view, the role of CGE has increased and contributed to the assessment of near to middle term impacs of climate measures.	Rejected--space constraints
10983	6	10	44	10	44	The term of "IAMs" is not defined.	Accepted
9838	6	11	11	11	15	Chapter 6.2.2 talks about the uncertainties. This is a limitation that should be discussed later, the same argument holds as mentionned for feasibility. I am not sure the word "prediction" should be used here unless it is made clear that the modeling efforts described do not involve making predictions or forecasts. The text is just discussing scenarios. Therefore, I don't think that the discussion of uncertainty in this sub-section 6.2.2 is really needed or relevant if one is not discussing forecasting. It certainly can be shortened.	Taken into account--we have added a footnote explaining differences in connotations between scenarios, projections, predictions, and forecasts and have added a citation that discusses this distinction
4194	6	11	11	11	35	The interpretation of the model emsemble in this chapter should be compared with those in WG-I, the case of GCMs. The latter represents the ranges of parameterization of climate science based on the similar theoretical formulations while the former often includes the variety of social context as well as the uncertainties in technological assessment.	Rejected--space constraints
8624	6	11	15			I am not sure the word "prediction" should be used here unless it is made clear that the modeling efforts described do not involve making predictions or forecasts. The text is just discussing scenarios. Therefore, I don't think that the discussion of uncertainty in this sub-section 6.2.2 is really needed or relevant if one is not discussing forecasting. It certainly can be shortened.	Taken into account--we have added a footnote explaining differences in connotations between scenarios, projections, predictions, and forecasts and have added a citation that discusses this distinction

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13130	6	11	26	11	35	This is extremely vague and not convincing. Using two scenarios instead of one would "contain information" about uncertainty , a truism, but would tell little about what uncertainties the differences between these two scenarios reflect, how complete is the coverage of uncertainties, how the differences can be interpreted and what interpretation are clearly out of reach. Or in other words, if there is full, formal information about uncertainty in one end of a range and no information at all in the other end, this statement only says that we are not in the "no information at all" part of the range, without suggesting what that means for using the scenario ensemble to represent uncertainties (in some form). Also, nothing guarantees that the distribution of scenarios has anything to do with "actual" distributions of uncertainties. On the contrary, most modelling teams are likely to aim at "best guess" parameter values, suggesting that tails of parameter distributions are likely to be under represented (the huge amount of possible parameter combinations guarantees heterogeneity for results even in this case). I suggest that any uncertainty related conclusions that are drawn from the scenario ensemble are worded extremely carefully throughout the chapter.	Taken into account--we have added a footnote and citation that discusses this point (Morgan and Keith, 2008)
7671	6	11	3	11	4	The text says "[…] these models typically assume market behavior […]". Perhaps a remark should be made that markets are virtually always assumed to be efficient. IAM's take rarely (if at all) e.g. information asymmetry, search frictions or market power into account.	accepted--text revised
9839	6	11	36			Here we return to discussing model "infeasibility" again - it is repetitious and over-emphasized as an issue.	taken into account--infeability discussions in other sections have been shortened and blended into this section
8625	6	11	36			Here we return to discussing model "infeasibility" again - it is repetitious and over-emphasized as an issue.	taken into account--infeability discussions in other sections have been shortened and blended into this section
16691	6	11	4			Make explicit that the market behavior, with the policy cases frequently employing a price on GHG emissions as the incentive.	Rejected--we make this point later in the section
9840	6	11	46			Again, the discussion of model infeasibility should NOT be allowed to "arise repeatedly". And it has very limited, not important, implications for "our understanding of real world feasibility." For example, one reason why some modeling groups stress scenario infeasibility for the RCP 2.6 scenario is because they limit the rate of efficiency improvements far too strongly on the demand-side; I think to less than 1.5% per year. If 3-4% per year efficiency improvements were allowed, then no scenario might have been infeasible.....	taken into account--infeability discussions in other sections have been shortened and blended into this section
8626	6	11	46			Again, the discussion of model infeasibility should NOT be allowed to "arise repeatedly". And it has very limited, not important, implications for "our understanding of real world feasibility." For example, one reason why some modeling groups should scenario infeasibility for the RCP 2.6 scenario is because they limit the rate of efficiency improvements far too strongly on the demand-side; I think to less than 1.5% per year. If 3-4% per year efficiency improvements were allowed, then no scenario might have been infeasible.....	taken into account--infeability discussions in other sections have been shortened and blended into this section
10984	6	11	6	11	8	In this sentence, the year 2020 is illustrated as a turning point from the medium-term to the long-term. Why is the year 2020 considered as such a turning point? It should be clarified.	Accepted--2020 deleted from sentence.
7672	6	11	11			On interpreting scenario ensembles, the risk for systematic bias should be also noted. Many IAM's share the same theoretical backgrounds and solution concepts. If a real-life feature (market inefficiency, non-market factors, uncertainty/limited foresight etc.) is not captured by the models, the whole ensemble is biased to some extent. (This is actually mentioned briefly in the end of section 2.3.6.2.)	Taken into account--model shortcomings discussed in section 6.2.1

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16692	6	12				Can model descriptions be moved to an appendix? Does the policymaker need this? It is interesting and important, but a bit of detail that most will gloss over.	Accepted--section condensed
16693	6	12		13		Have you answered the following questions clearly enough: 1) Are models "valid"? 2) Do the results make sense? 3) How do the model results compare to our understanding or experience of the real world?	Taken into account--we try to address these, but due to space constraints are perhaps unable to elaborate to the reviewer's satisfaction
9842	6	12	11			This sub-section should be moved forward so that the structures of the models are discussed before results.	Taken into account--this section does come before the results in section 6.3 but comes after the executive summary which can't be moved
13131	6	12	11	14	4	As the chapter is currently some 20 pages longer than it should be, I suggest this section on the tools is cut down considerably. The descriptions on the trade, foresight etc dimensions of the models should be summarized much more concisely and more references could be given instead. I don't see why it would be necessary to give this much detail when the results are anyway analysed mostly on the level of the full scenario ensemble. I also suggest removing table 6.1. completely - similar information could be given together with the concise descriptions, naming model examples and giving references. Finally, if the authors insist on keeping the table in, it needs to be clarified and made consistent. For example, for two models there is trade in "primary energy, secondary energy and energy goods". How are energy goods defined, if they don't fall under primary or secondary energy? The different options for model flexibility are also unclear, potentially also to the authors as different interpretations appear to exist (i.e. models that are rather similar, and should include the same flexibility options, don't). This also applies to cost measures.	Agreed--section condensed
8628	6	12	11			This sub-section should be moved forward so that the structures of the models are discussed before results.	Taken into account--this section does come before the results in section 6.3 but after the executive summary which can't be moved
9843	6	12	20			One implication of their being two kinds of models with two very different kinds of macro-economic outputs seems to me to be that economic results can not be compared validly at all between the two types of models. Yet, I believe at many points later in the chapter, cost results from these two different types of models are compared and even placed in the same figures. Furthermore, to the extent that different model runs assume different discount rates, it is a simple conceptual point that these results can not be compared either. Please check if different discount rates were ever used in the results that you lump together.	Noted--we agree that the model ensembles from which we take these scenarios involve very different models, but it is outside of the chapter's scope to conduct a comprehensive comparison across all the models included in these model intercomparison exercises. These exercises themselves do, however, provide such comparisons

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8629	6	12	20			One implication of their being two kinds of models with two very different kinds of macro-economic outputs seems to me to be that economic results can not be compared validly at all between the two types of models. Yet, I believe at many points later in the chapter, cost results from these two different types of models are compared and even placed in the same figures. Furthermore, to the extent that different model runs assume different discount rates, it is a simple conceptual point that these results can not be compared either. Please check if different discount rates were ever used in the results that you lump together.	Noted--we agree that the model ensembles from which we take these scenarios involve very different models, but it is outside of the chapter's scope to conduct a comprehensive comparison across all the models included in these model intercomparison exercises. These exercises themselves do, however, provide such comparisons
9844	6	12	31			The concept of "the area under the marginal abatement cost function" needs to be explained for a general reader. But, in addition, marginal costs would not seem relevant to calculating the average cost of abatement, which is what is needed. (Average cost per unit of abatement times total abatement equals total cost of abatement, correct?) What should be the additional value of the use of a marginal cost function be justified in this type of cost comparison?	Taken into account--section condensed and details eliminated
8630	6	12	31			The concept of "the area under the marginal abatement cost function" needs to be explained for a general reader. But, in addition, marginal costs would not seem relevant to calculating the average cost of abatement, which is what is needed. (Average cost per unit of abatement times total abatement equals total cost of abatement, correct?) How can the use of a marginal cost function be justified in this type of cost comparison?	Taken into account--section condensed and details eliminated
7673	6	12	33	12	34	The text contrasts "feedbacks to the full economy" and "more possibilities for substitution" in GE models. These are not contrasting things, as the substitution possibility is one kind of a feedback effect. The two clauses ("On one hand […]" and "On the other hand [...]") should be reformulated. Are there examples and references for the negative feedback effects?	Noted
9950	6	12	40		41	Whether models with perfect foresight will lower the economic costs depends on the value of discounting rate. A lower discounting rate will result in a increasing costs. So please pay attention to the statement.	Taken into account--section condensed and details eliminated
7674	6	12	41	12	41	The text says "[…] a model with perfect foresight will have lower economic costs […]". Perhaps this should be interpreted as that a perfect foresicht model gives a lower bound for costs, because perfect foresight is not possible in reality.	Noted
7675	6	12	41	12	41	Why a carbon tax is mentioned specifically? The same observation holds for a quantitative emission limit, a forcing or a temperature target (although these might need some sort of foresight, at least in form of expectations).	Accepted--"tax" changed to "policy"
9845	6	12	46			With respect to the statement that the level of investment is determined by a fixed savings rate, how is this rate determined? Can a low savings rate constrain the amount of investment per year in renewable energy, for example, below what is needed to meet a given climate target? Could a low savings rate contribute to "infeasibility" as discussed earlier in the chapter? If not, why not? This seems like another very important assumption, the level of the savings rate, therefore more discussion of its role in different kinds of macro-economic modules within IAMs appears necessary.	Taken into account--section condensed and details eliminated
8631	6	12	46			With respect to the statement that the level of investment is determined by a fixed savings rate, how is this rate determined? Can a low savings rate constrain the amount of investment per year in renewable energy, for example, below what is needed to meet a given climate target? Could a low savings rate contribute to "infeasibility" as discussed earlier in the chapter? If not, why not? This seems like another very important assumption, the level of the savings rate, therefore more discussion of its role in different kinds of macro-economic modules within IAMs appears necessary.	Taken into account--section condensed and details eliminated

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9846	6	12	47			How is the marginal propensity to invest and consume calculated in models that use those parameters? What is the implication of those methodologies on the mitigation scenarios?	Taken into account--section condensed and details eliminated
8632	6	12	47			How is the marginal propensity to invest and consume calculated in models that use those parameters? What is the implication of those methodologies on the mitigation scenarios?	Taken into account--section condensed and details eliminated
9841	6	12	9			Good - finally the dependence of scenario infeasibility on input assumptions is mentioned, but not explained in sufficient detail. Clearly, the entire discussion of infeasibility should be put after most important results about feasible scenarios are presented, and it should be reorganized and shortened. Let's talk about what is feasible first.	Noted
8627	6	12	9			Good - finally the dependence of scenario infeasibility on input assumptions is mentioned, but not explained in sufficient detail. Clearly, the entire discussion of infeasibility should be put after most important results about feasible scenarios are presented, and it should be reorganized and shortened. Let's talk about what is feasible first.	Noted
9847	6	13	1			Perhaps a simple figure showing this comparison in investment trajectories between these two different types of models would give the reader a better feel for how big the difference might be. Otherwise, it will be hard even for experienced modelers to get a sense of the differences between the two different models, and how different the mitigation trajectories might be. I.e. what difference would it make for policy makers which kind of model is used?	Taken into account--section condensed and details eliminated
8633	6	13	1			Perhaps a simple figure showing this comparison in investment trajectories between these two different types of models would give the reader a better feel for how big the difference might be. Otherwise, it will be hard even for experienced modelers to get a sense of the differences between the two different models, and how different the mitigation trajectories might be. I.e. what difference would it make for policy makers which kind of model is used?	Taken into account--section condensed and details eliminated
9951	6	13	14		44	In IAM models, technological change is one of the most important factors determining the emission project. The change rate of technology influences future emission substaintially. But in these two paragraphs, issues mentioned are mainly concerned about CGE models, factors in IAMs, which ae not CGE-based, are fosuced on little.	Taken into account--section condensed and details eliminated
9848	6	13	15			The sentence beginning here does not have two different kinds of models explicitly mentioned. Please re-write.	Taken into account--section condensed and details eliminated
8634	6	13	15			The sentence beginning here does not have two different kinds of models explicitly mentioned. Please re-write.	Taken into account--section condensed and details eliminated
9849	6	13	18			"nested CES structure" is not explained. In general, this parts needs to be expanded so that the economic modeling approaches can be explained better, or eliminated.	Taken into account--section condensed and details eliminated
8635	6	13	18			"nested CES structure" is not explained. In general, either is page or two needs to be expanded so that the economic modeling approaches can be explained better, or eliminated. No one but an economist will be able to understand this section entitled "model flexibility". In fact, I am not sure it would be clear to anyone, especially the last few sentences. At the very least, please re-write - I don't know what is saying about fossil fuel constraints and their impact.	Taken into account--section condensed and details eliminated
9090	6	13	20			Why "uranium in the case of nuclear" is cited as example in this context ? And what is the specific meaning of "in the case of nuclear" ? Detailed explanation is required.	Taken into account--section condensed and details eliminated
9567	6	13	26			Please, delete the before how.	editorial

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9850	6	13	28			First of all, the first sentence as stated in economist's terminology obscures the huge importance of this assumption in some models - that the entire economy (presumably net of the energy sector, and others(?)) can be represented by just one or a few CES production functions. The text should make it clear that this means one can not distinguish between economic activity in the household, commercial, government, transportation, or industrial sectors, or within each sector. It is all one big aggregate "glob". Yet, the next few sentences makes is sound like a model with just one economic sector, implicitly assuming perfect substitutability across the economy, has some advantage relative to allocating factors of production. Of course, just the opposite is true. The more aggregate the economy as modeled, the more unrealistic and, therefore, inaccurate the results of calculations will be. This is particularly true for the cost of mitigation results. I believe that these highly aggregated economic models will dramatically underestimate the costs of climate change mitigation for the same reasons the text cites. If I am right, this weakness of these models must be honestly described. In their defense you might also mention the problems with running models with more highly disaggregated economic sectors. The same is true for lack of sufficient regional disaggregation. The related point that should be mentioned is that not having sufficient end-use or demand-side detail means that the degree of possible substitutability between many demand-side technologies will be greatly overestimated (implicitly). Also, more aggregation raises very serious issues about how to project changes in the economy into the future relative to possible changes in culture and lifestyles.	Taken into account--section condensed and details eliminated
8636	6	13	28			First of all, the first sentence as stated in economist's terminology obscures the huge importance of this assumption in some models - that the entire economy (presumably net of the energy sector, and others(?)) can be represented by just one or a few CES production functions. The text should make it clear that this means one can not distinguish between economic activity in the household, commercial, government, transportation, or industrial sectors, or within each sector. It is all one big aggregate "glob". Yet, the next few sentences makes is sound like a model with just one economic sector, implicitly assuming perfect substitutability across the economy, has some advantage relative to allocating factors of production. Of course, just the opposite is true. The more aggregate the economy as modeled, the more unrealistic and, therefore, inaccurate the results of calculations will be. This is particularly true for the cost of mitigation results. I believe that these highly aggregated economic models will dramatically underestimate the costs of climate change mitigation for the same reasons the text cites. If I am right, this weakness of these models must be honestly described. In their defense you might also mentioned the problems with running models with more highly disaggregated economic sectors. The same is true for lack of sufficient regional disaggregation. The related point that should be mentioned is that not having sufficient end-use or demand-side detail means that the degree of possible substitutability between many demand-side technologies will be greatly overestimated (implicitly). Also, more aggregation raises very serious issues about how to project changes in the economy into the future relative to possible changes in culture and lifestyles.	Taken into account--section condensed and details eliminated
9952	6	13	28			The title for the paragraph should be "…and GHG detail".	Agreed--text revised
7676	6	13	3	13	44	The text's level of technicality might be reconsidered. Discussion on coverage and foresight is relatively accessible, but the paragraphs on trade, flexibility and detail dive into how production functions are nested. Moreover, production functions and nesting are explicitly applied only in GE models. Perhaps the text should be on a more general level, and focus on how different assumptions relate to the real world and what implications the assumptions have.	Taken into account--section condensed and details eliminated
12308	6	13	44	13	44	Please consider to explain the column "Optimization/Simulation" similar to the other columns in Table 6.1.	This table has been removed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9851	6	14				How did you select the models? Please provide search terms and information on the selection process in a footnote. Moreover regional scenario models are left out. On page 10 you mention that regional models are left out, but climate change is relevant for most of the decision makers on a regional scale only because their individual influence is restricted to a specific region, be it as a politician or a business practionner.	This table has been removed
9403	6	14				As for explanation of the AIM-Enduse model, please write "6 GHGs" instead of "5 GHGs". In addition, please remove information of (energy-related) when counting sectral numbers. Anyway, this table is unclear, for example, definitions of model flexibility and how to count sectors and regions are unclear. Please carefully check information of original models in Table 6.1.	Taken into account--all model information was provided by the individual model teams in a questionaire included as part of their submission to the AR5 data base. This table has been removed.
13760	6	14				Please provide references for the different models. Why is IMAGE not included - it is explicitly included in Fig. 6.26?	Taken into account--only model results that were submitted to the AR5 data base were included in the table. This table has been removed.
9953	6	14				Please give more details about the models including values of key parameters, such as the value for economic growth rate, so that others can follow the work. At least, papers which introduce the detailed structure of each model should be listed. Why those models are selected for scenario generation? Maybe it's better to explain the criterion for model selection.	Taken into account--all model information was provided by the individual model teams in a questionaire included as part of their submission to the AR5 data base. Due to space constraints, we were not able to provide model details, but references are provided. this table has been removed
7677	6	14	1			The model comparison tabe is good, but the data in the flexibility and detail-level columns can be interpreted so loosely that the information loses its meaning. From what I know of the MESSAGE and TIAM (though not of the ECN's version) models, they should have roughly the same amount of flexibility and covered sectors. From what understand, MESSAGE is here assumed to cover one large energy sector, while TIAM differentiates between energy subsectors. (If ECN's model is heavily reformulated from the original TIAM, this comment does not necessarily apply.)	Taken into account--all model information was provided by the individual model teams in a questionaire included as part of their submission to the AR5 data base. This table has been removed.
8348	6	14	1			There are other models such as LEAP and MARKAL/TIMES model. I suggest a paper. Bhattacharyya S.C. and G.R. Timilsina, (2009). Energy Demand Models for Policy Formulation - a comparative study of energy demand models. The World Bank, pp.91-92.	Taken into account--all model information was provided by the individual model teams in a questionaire included as part of their submission to the AR5 data base. This table has been removed.
8637	6	14	1			I don't think that Table 6.1 is filled in in a consistent way across models. For example, check the column headed "sectoral, regional…" It should be clear how many sectors are represented for the energy sector separately from the non-energy sectors in the same format. Similarly, some terms are not clear e.g. "energy system cost markups". I never saw that phrase used before.	Table has been removed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16694	6	15				It would be helpful to discuss scenarios/model results that examined the OECD making aggressive reductions now while developing countries continued with BAU emissions until 2040, at which time they reduced. Do we achieve safe stabilization levels? This would be helpful for people to understand.	There are many delayed participation scenarios in the literature, which are indeed an important component of the story. These will be discussed in the chapter, but likely not in the baseline section. At this stage we are still determining the most appropriate placement within the chapter's structure.
6906	6	15				Please check RCP discussion, especially figure content etc., to be consistent between WGs.	The data shown in the figure is from the published RCP results (Van Vuuren et al, 2011). This reference will be clarified in the chapter. Every effort will be made to ensure consistency across working groups with respect to these results.
9217	6	15	1	15	1	It should be notd that the "concentration stabilization" is not a likely future and this has implication on the scale of emission reduction policies. In (T. Matsuno, K. Maruyama and J. Tsutsui “Stabilization of atmospheric carbon dioxide via zero emissions-----An alternative way to stable global environment”. Part 1 and 2 In Proceedings of Japan Academy Ser. B, Vol. 88, No.7 (July, 2012),p 368-395.), the authors critically examine the traditional “stabilization” concept in which the atmospheric CO2 concentration and corresponding temperature are held constant for many centuries to a millennium. They claim that such long-term constancy of concentration and temperature is not a likely future state. Instead they propose “zero-emissions stabilization” in which emissions wi be diminished close to zero, and after that the concentration will decrease approaching the final equilibrium state for which the temperature rise can be made much lower to avoid the risk of sea level rise. Another advantage of the zero-emissions stabilization strategy is that emissions in the near future can be made larger compared with ordinary stabilization pathways under the same temperature rise constraint. This would be beneficial to respond to current socio-economic needs. These points are shown by simple model calculations for illustrative cases.	In the baseline section of this chapter we are summarizing what is assumed in the literature about the evolution of emissions in the absence of policy. In subsequent sections we describe stabilization scenarios. It is the mandate of the chapter and indeed of WGIII to describe the implications of stabilization as outlined in the UNFCCC in terms of concentrations / forcing. However, it is an important point that this not the only way to formulate a policy goal. A scenario in which emissions are required to be reduced to zero in the long run would indeed have different results, although it should be noted that many scenarios in the literature have negative emissions by the end of the current century in order to achieve a stringent concentrations / forcing target in the year 2100. The author team will consider where to discuss alternative formulations such as a target of zero emissions in the long run.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9853	6	15	21			The mean of RCP should be explained. Also, the text should explain how the RCP trajectories on Figure 6.1 were computed. Then I think a full paragraph is required to explain why there is such a vast range even for the set of baseline scenarios in Figure 6.1. Perhaps this could be illustrated by providing a partial or representative table showing how different many of the key input parameters are from model to model. This is where a fairly complete list of the types of input assumptions and drivers should be provided so the reader can understand why the results even for the baseline scenarios can be so different from each other. Otherwise, I find a figure like Figure 6.1 to be a fairly meaningless way of presenting results. The naive reader would say to themselves "these modeling teams can not even agree on a reasonably similar baseline scenario for comparison purposes to the mitigation scenarios. I wonder what a huge spread will be represented by mitigation scenarios". Therefore, if net costs are calculated by comparing the total cost of a baseline scenario to a type of mitigation scenarios, those differences will be "all over the map", and, therefore, may be seen as meaningless. Perhaps making comparisons between baseline and mitigation scenarios in a more disaggregated fashion would help convince the reader that the net costs that result are meaningful.	The data shown in the figure is from the published RCP results (Van Vuuren et al, 2011). This reference will be clarified in the chapter. As for the wide variation across model scenarios in the baseline emissions path, this is a key observation. The intent of Section 6.3.1 is to illustrate the drivers of the spread (as discussed in 6.3.1.3 and shown in Figure 6.5, these are per capita income growth rates and energy intensity parameters) and to emphasize that uncertainty about these two drivers, as well as other aspects of the baseline, are an important component of uncertainty about the ultimate costs of stabilization. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
8639	6	15	21			The mean of RCP should be explained. Also, the text should explain how the RCP trajectories on Figure 6.1 were computed. Then I think a full paragraph is required to explain why there is such a vast range even for the set of baseline scenarios in Figure 6.1. Perhaps this could be illustrated by providing a partial or representative table showing how different many of the key input parameters are from model to model. This is where a fairly complete list of the types of input assumptions and drivers should be provided so the reader can understand why the results even for the baseline scenarios can be so different from each other. Otherwise, I find a figure like Figure 6.1 to be a fairly meaningless way of presenting results. The naive reader would say to themselves "these modeling teams can not even agree on a reasonably similar baseline scenario for comparison purposes to the mitigation scenarios. I wonder what a huge spread will be represented by mitigation scenarios". Therefore, if net costs are calculated by comparing the total cost of a baseline scenario to a type of mitigation scenarios, those differences will be "all over the map", and, therefore, may be seen as meaningless. Perhaps making comparisons between baseline and mitigation scenarios in a more disaggregated fashion would help convince the reader that the net costs that result are meaningful.	The data shown in the figure is from the published RCP results (Van Vuuren et al, 2011). This reference will be clarified in the chapter. As for the wide variation across model scenarios in the baseline emissions path, this is a key observation. The intent of Section 6.3.1 is to illustrate the drivers of the spread (as discussed in 6.3.1.3 and shown in Figure 6.5, these are per capita income growth rates and energy intensity parameters) and to emphasize that uncertainty about these two drivers, as well as other aspects of the baseline, are an important component of uncertainty about the ultimate costs of stabilization. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9955	6	15	25		26	It's not easy for readers to know what does RCP scenario 2.6 or 4.5 mean. Maybe a table should be added here to explain the implications of RCP scenarios.	The data shown in the figure is from the published RCP results (Van Vuuren et al, 2011). This reference will be clarified in the chapter.
9852	6	15	5			section 6.3.1.1 should be moved into a "results" section, but the concept of a scenario should be clarified up-front. B I think that line 19 which talks about "best-guess" pathways for key drivers is not appropriate, because it conflicts with the idea stated on line 17 above that it is not meaningful to assign probabilities to driver or emissions pathways. A best guess says something about probabilities. Please fix this to make everything consistent. A scenario is just a set of assumptions and projections, independent of their probability of occurence. I also think it would be accurate to point out that each modeling team gets to choose its own set of assumptions for their baseline and RCP-x scenarios, so there is little to no consistency in key drivers assumed or calculated between modeling teams.	This is an important point. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
8638	6	15	5			section 6.3.1.1 should be moved into a "results" section, but the concept of a scenario should be clarified up-front. B I think that line 19 which talks about "best-guess" pathways for key drivers is not appropriate, because it conflicts with the idea stated on line 17 above that it is not meaningful to assign probabilities to driver or emissions pathways. A best guess says something about probabilities. Please fix this to make everything consistent. A scenario is just a set of assumptions and projections, independent of their probability of occurence. I also think it would be accurate to point out that each modeling team gets to choose its own set of assumptions for their baseline and RCP-x scenarios, so there is little to no consistency in key drivers assumed or calculated between modeling teams.	This is an important point. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
16346	6	15	8	15	8	Please clarify: do you mean "no explicit climate policy intervention" ? (see my general comment on section 6.3.1 for more information)	This is an important point. While different models may frame their baselines differently, the cleanest conceptual definition in the context of integrated assessment of climate stabilization is a scenario in which the GHG externality is neither implicitly nor explicitly priced. We will clarify the text.
6496	6	15	1			Section 6.3 is divided smaller than other sections. So, this section should be significantly reduced as same volume as other sections.	The structure of the chapter is being revised to best cover the material within the prescribed outline.
6505	6	15	1			6.3.2.6 (Solar radiation management and stabilization scenarios) and 6.9.2 (Solar radiation management) had better to be aggregated. Because they are similar in the content.	The structure of the chapter is being revised to best cover the material within the prescribed outline.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16345	6	15	4			The concept of "baseline" needs to be explained in more detail with regard to the inclusion of policies related to sustainable development. There are 2 related issues: - it needs to be made clear that baselines may include policies that contribute to mitigation as long as these are not directed at climate change mitigation alone, in particular policies and measures designed in a broader context of sustainable development are included in baselines. This is not a future "with no policy intervention or with only specific policies" (as currently suggested in 6.3.1.1.); I would rather understand baselines in AR5 as a future with "no new policy addressing climate change alone". If it is not so, a clarification is even more important. - The limitations of the use of baselines should also be explained. I would indeed expect that there are scenarios in which efforts to tackle climate change and its impacts are so strongly integrated with other socio-economic objectives that it is hardly possible to find an appropriate baseline - that is, to remove the climate policies and have all the other policies remain unchanged. The reasons for such an integrated thinking of sustainable development and climate change have been described in several papers, and it has even been argued that "sustainable development may offer a significantly more fruitful way to pursue climate policy goals than climate policy itself" (Robinson et al., Ambio, vol 35, pp 2-8, 2006). I think that this is not just something that can be noted in a separate chapter, but a real limitation to the "baseline + climate policy" approach.	This is an important point. While different models may frame their baselines differently, the cleanest conceptual definition in the context of integrated assessment of climate stabilization is a scenario in which the GHG externality is neither implicitly nor explicitly priced. It is true that policies with other objectives (such as reducing local air pollution or increasing energy security) will influence GHG emissions. The extent to which such non-GHG policies are incorporated into climate scenario baselines is model-specific. The author team is working on ways to better incorporate the concept of sustainable development integrated with climate policy into the discussion.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16348	6	15	21			I noticed that scenario data is preliminary, but I think that the text will need to be adapted to the additional data, especially regarding low emission baselines. The emissions currently shown are way above the lowest cases shown in IPCC SRES (B1). Would this mean that such scenarios are absent from the recent literature ? I think that the text will need more emphasis on the role of sustainable development in achieving low emissions (how these low baselines may help achieving stabilisation).	This section is focused on an objective assessment of the results in published baseline scenarios. The author team is considering whether to include the SRES scenarios on the graphic as a reference point, but in any case it is true that in the assessed literature there are no so-called "low-emissions baselines." It can be difficult to ascertain, both as an author and reader of the assessment, whether particular instances (such as indirect GHG reductions due to non-climate SD policies) don't appear in the literature because they are unlikely or because they were simply neglected. As for scenarios in which GHGs are reduced due to combined climate-SD policies, these are not baseline scenarios as conceptualized here and are not reported in this section. The author team is working on ways to better incorporate the concept of sustainable development integrated with climate policy into the discussion.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15219	6	16				Figures 6.1 and 6.2 are difficult to understant due to too many lines.	The author team is working on ways to optimize the visual representation of scenario data. In some cases the "spaghetti" format is the "least bad" approach: in this format, particularly with all scenarios shown in the same color, the point is not to display the characteristics of individual data series (which is indeed very difficult) but rather to display the characteristics of the ensemble as accurately as possible. In this sense it is superior to a shaded range or "box and whisker" alternatives because it conveys to the reader the true frequency of reported data. This is potentially important in the context of baseline emissions paths because the distribution is not uniform (e.g. the upper end of the range has only a few instances). However, it is true that the chapter needs to be clearer about how to interpret frequency in scenario data. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
4195	6	16				Interesting for researchers in this field, but very difficult for other readers to get the information from this. Almost no relationship is observed between energy intensity and per capita income, even in USA. I think the convergence of EI of countries along the time horizon would give useful information, if it exists.	The author team is considering dropping this figure due to its limited added value to the chapter. In fact countries do not converge in EI over time: EU and Japan are much lower than USA and Canada, due to a variety of factors. A key question for baselines is where developing countries will end up on this spectrum.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9410	6	16	17			Instead of drawing this figure in fossil and industrial CO2 emissions, it is recommended to draw in GHG emissions in order to keep it consistent with Figure 6.2.	The author team is considering what the best metric is for displaying emissions paths. CO2-e including Kyoto gases converted using GWPs is more inclusive (but not totally consistent with Figure 6.2 and 6.3 since the non-gas forcing agents cannot be included), but can obfuscate the growth in energy-related emissions, which is the dominant source.
6263	6	16		16		Remove "ORNL" from both graphics and replace with "History" who compiled these data are not the important point to convey in this graphic.	Agreed.
9411	6	17				This figure is not informative. It is too difficult to see consistency between non-OECD and OECD within the same scenario. For example, the scatter-plot figure (OECD in X-axis and non-OECD in Y-axis) in different years is much meaningful.	The author team is working on ways to improve this graphic. A scatter plot format could indeed be a good alternative in this case.
13132	6	17	15	17	17	Or more precisely, there is evidence that *incremental* change may not be enough, if all the other assumptions remain in place. One can easily imagine baseline scenarios in which emissions would dip due to, for example, a revolutionary, low cost carbon free technology emerging. As the baselines are often created as a reference point for mitigation scenarios (as was pointed out earlier in the draft), such a baseline would be somewhat purpose defeating (and optimistic, of course) and it's therefore unsurprising that no such baselines have been observed in the literature. This also indicates that the lack of such baseline can't by itself be considered an indication of it being impossible (or even less likely than other baseline) - that would suggest taking the assumption of the analyst (i.e. that it's not worthwhile to create such a baseline scenario) and presenting it as a conclusion.	It is one of the key messages of this section that among published baseline scenarios, notional forcing targets are not achieved. The author team is working on ways to emphasize this point while also being clear that the range of assumptions made in published baseline scenarios likely under-represents the true range of uncertainty in key input parameters (both related to growth and technology costs). A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16695	6	17	15		17	This is very important point and should be given emphasis somehow.	It is one of the key messages of this section that among published baseline scenarios, notional forcing targets are not achieved. The author team is working on ways to emphasize this point while also being clear that the range of assumptions made in published baseline scenarios likely under-represents the true range of uncertainty in key input parameters (both related to growth and technology costs). A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
8640	6	17	15			Again, the sentence starting "Thus there is strong evidence…" is a very important conclusion which comes out of the blue, since the kinds of technological change refered to is never described in any detail. And the term "policy intervention" on line 16 should be explained also. Finally, because the "strong evidence" refers to the wide range of baseline results in Fig. 6.1, the reader will likely be somewhat unclear as to the basis for this sweeping conclusion. (I agree with the content of the conclusion, of course.) But a reader might also ask the question, are there other reasonable input assumptions for a baseline case that would make the emissions trajectory more compatible with RCP4.5, for example, if oil prices were $500 per barrel in 2100, etc....	It is one of the key messages of this section that among published baseline scenarios, notional forcing targets are not achieved. The author team is working on ways to emphasize this point while also being clear that the range of assumptions made in published baseline scenarios likely under-represents the true range of uncertainty in key input parameters (both related to growth and technology costs). A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
5857	6	17	18	17	21	Please delete "other" preceding "developing countries" else you mean Russia, China or India to be DCs, too.	The labeling of countries in terms of categories of development, etc. will be made consistent throughout the report.
8641	6	17	18			Does the phrase "over the century" mean the cumulative amount, the annual amount, or both?	Cumulative. Text will be clarified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12610	6	17	18	17	27	Into the discussion the authors present as non OECD countries only China, Russia, Brazil, India, South Africa and other countries. Which ones? . For the trayectories shown into the figure of these countries, in the future their future emission will be greater than the OECD countries. Countries like the SIS would not be included, never their future emissions would be similar to the OECD countries.	The non-OECD category includes all countries not explicitly in the OECD. While it is true that SIS emissions by themselves would likely not exceed OECD in any baseline projection, they are included in the non-OECD total in most models. Regional definitions will be standardized throughout the report with regions explicitly defined in an appendix.
11417	6	17	18	17	27	The assertion that in all baseline scenarios, the majority of emissions over the 21st century will come from non-OECD regions and countries needs to be explained more clearly in terms of what the assumptions are underlying such assertion. Absent a clear explanation of the assumptions for this assertion, such a bare assertion could be used in a non-scientific and political way in order to push specific policy agendas or approaches in the context of international policymaking discussions and negotiations on climate change that could effectively absolve developed countries of any further mitigation commitments and increase the pressure on developing countries to undertake increased mitigation actions. Furthermore, this paragraph does not fully nuance the assertion - what it seems to do is to project the current rates of population growth and economic growth of non-OECD regions in a linear fashion into the future and then concludes that because of these, non-OECD countries will therefore be the biggest contributors to future emissions.	The statement is an objective assessment of the results in published baseline scenarios. As discussed in the section, the key drivers for these projections are per capita income growth rates and energy intensity parameters. Models use a variety of methodologies for country-level baseline projections, which are in nearly every case more sophisticated than simply assuming current growth rates persist indefinitely. For example, Figure 6.5 shows how the average growth rates over the next four decades for certain key regions are projected relative to the observed history of the preceding four decades.
12609	6	17	2	17	4	Does x axis represent the total emissions on billions tons of CO2 or the increment on total emisssions of CO2 for OECD and non OECD countries? Please clarify.	The x axis represents time. The y axis represents total emissions. The author team is working on ways to improve this graphic.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9854	6	17	5	17	17	It should be explained, why the unit the W/m2 is chosen and how it is correlated to ppm and also oC. It is not familiar to many of the potential readers.	W/m2 is the unit for radiative forcing. We can add a reference to WGI for a definition of this quantity. The targets refer to total radiative forcing from all agents (not just CO2), hence the comparison is given to concentrations of CO2-equivalent. We can also add a reference to WGI definining CO2-equivalent concentrations. Neither concentrations nor forcing can be uniquely translated to temperature: this an uncertain and dynamic relationship. Again, we refer to WGI.
10391	6	17	8	17	8	There is a mistake for the unit about the radiative forcing "2.6 W.m2".	Noted.
16696	6	18				The graph seems to suggest that economic growth is the biggest variable or determinant of CO2 emissions -- therefore if we want to lower emissions we need to lower economic growth. This is obviously wrong but unless this is clarified, the misunderstanding will persist. Should therefore explain the graph is a baseline and if policy is enacted which creates incentive for low emitting technology this relationship (econ & emissions) can be significantly changed. We know this from experience.	In fact the graph refers to rates of change, not overall contributions to emissions. The observation is that uncertainty about economic growth is a major driver of uncertainty about baseline emissions. The graph does not suggest that carbon intensity is unimportant as a driver of emissions, only that it does not change much over time in published baseline scenarios. Still, it is an important point that mitigation chiefly involves reducing carbon intensity more so than any of the other Kaya drivers. The author team is working on ways to emphasize this point both in the text and with one or more figures.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9855	6	18	10			Note that while there is a reasonable discussion of the carbon intensity issue between models, one might think that the exception noted on lines 14-16 would be the norm.	This section is focused on an objective assessment of the results in published baseline scenarios. The author team is working on ways to clarify that the range of assumptions made in published baseline scenarios likely under-represents the true range of uncertainty in key input parameters (both related to growth and technology costs). It can be difficult to ascertain, both as an author and reader of the assessment, whether particular instances (such as very cheap renewables) don't appear in the literature because they are unlikely or because they were simply neglected. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
13133	6	18	10	18	11	I think it's noteworthy that there is not a single documented "degrowth" baseline scenario, not even on the regional level. This is noteworthy also in terms of how well the existing baseline scenario ensemble might capture the "full", relevant baseline scenario space.	This section is focused on an objective assessment of the results in published baseline scenarios. The author team is working on ways to clarify that the range of assumptions made in published baseline scenarios likely under-represents the true range of uncertainty in key input parameters (both related to growth and technology costs). It can be difficult to ascertain, both as an author and reader of the assessment, whether particular instances (such as "degrowth") don't appear in the literature because they are unlikely or because they were simply neglected. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8643	6	18	10			Note that while there is a reasonable discussion of the carbon intensity issue between models, one might think that the exception noted on lines 14-16 would be the norm.	This section is focused on an objective assessment of the results in published baseline scenarios. The author team is working on ways to clarify that the range of assumptions made in published baseline scenarios likely under-represents the true range of uncertainty in key input parameters (both related to growth and technology costs). It can be difficult to ascertain, both as an author and reader of the assessment, whether particular instances (such as very cheap renewables) don't appear in the literature because they are unlikely or because they were simply neglected. A clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
14396	6	18	17			does the statement about fossil carbon intensity reflect the new situation for natural gas with the development of fracking?	Most models have taken into account the latest information about resources and extraction technology. This can be clarified here or perhaps better elsewhere in the chapter with respect to shale gas.
9412	6	18	19			Findings between fiture 6.5 and 6.6 are overlaping. To save space, these figures can be incorporated	The author team is considering dropping Figure 6.6 due to its limited added value to the chapter.
7678	6	18	20			Insert an axis label. The caption might be improved: "[…] growth rates of Kaya decomposition indicators between 2010 and 2050 […]" (i.e. swap the order of indicators and years).	OK.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9856	6	18	20			Figure 6.5 shows that some models decrease in energy intensities per $ of GDP at more than 3% per year, and some at more than 4% per year. I was not aware of any team or model result in this range. Please check and document and discuss which teams do this for the full 40 year period discussed. If these numbers are accurate to what extent is this net result, as discussed on page 19, the result of end-use efficiency improvements, and to what extent are these dramatic results (compared to history) the result of "structural changes in the composition of energy demand"? Given that most IAMs can only project structural change at a very aggregate level, as you say on line 18-19, are the rates of structural change shown as outliers in figure 6.5 reasonable? You say this is discussed in section 6.7, but I don't see such a discussion there. That section discusses risks and not the relationship between top down and bottom up assessments of energy intensity.	The reported data is accurate. The results refer to "net" declines, i.e. inclusive of both autonomous and price-induced effects and both structural change and end-use efficiency improvements. Many models do project faster rates of decline in "net" energy intensity for countries such as China and India than were observed in those countries in the past. It is not the goal of the chapter to report and diagnose results at the level of individual models, and unfortunately there is not space to delve into a decomposition of the various effects (nor is there typically sufficiently detailed reporting to do so across models). It is difficult to say whether these results are "reasonable" - the section is focused on an objective assessment of the results in published baseline scenarios. We will correct the cross-reference.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8644	6	18	20			Figure 6.5 shows that some modeling teams (input?) decreases in energy intensities per $ of GDP at more than 3% per year, and some at more than 4% per year. I was not aware of any team or model result in this range. Please check and document and discuss which teams do this for the full 40 year period discussed. If these numbers are accurate to what extent is this net result, as discussed on page 19, the result of end-use efficiency improvements, and to what extent are these dramatic results (compared to history) the result of "structural changes in the composition of energy demand"? Given that most IAMs can only project structural change at a very aggregate level, as you say on line 18-19, are the rates of structural change shown as outliers in figure 6.5 reasonable? You say this is discussed in section 6.7, but I don't see such a discussion there. That section discusses risks and not the relationship between top down and bottom up assessments of energy intensity.	The reported data is accurate. The results refer to "net" declines, i.e. inclusive of both autonomous and price-induced effects and both structural change and end-use efficiency improvements. Many models do project faster rates of decline in "net" energy intensity for countries such as China and India than were observed in those countries in the past. It is not the goal of the chapter to report and diagnose results at the level of individual models, and unfortunately there is not space to delve into a decomposition of the various effects (nor is there typically sufficiently detailed reporting to do so across models). It is difficult to say whether these results are "reasonable" - the section is focused on an objective assessment of the results in published baseline scenarios. We will correct the cross-reference.
16347	6	18	21			I am surprised that Africa is not included at all. As it represents a substantial part of the World population, it could be a useful addition.	Reporting of regional results is constrained to a significant extent by the regional definitions used in individual models. Unfortunately many models do not separate Africa as a single region, thus it is not possible to isolate results for Africa across models.
8642	6	18	3			Good news - again a few key input assumptions are mentioned. But, again, this discussion should be consolidated with the previous discussion of input assumptions, and moved forward in the chapter so it appears before the results for baseline scenarios, not after.	This subsection is intended to be the place where discussion of input assumptions is consolidated. The author team is working on revising the flow of this section.
2226	6	19	25	22	21	1) From a policy maker or a business view this is very (too?) complicated for one of the most essential questions (stabilization) (acknowledged that the topic is very complicated!). Now, would there be a possibility of linking the radiative forcing values/categories to temperature increases. Why? First, most people want to link mitigation to stated temperature increase targets like in the Copenhagen Accord. Second, a lot of people can translate temperature increases better to impacts of warming (see Stern chart on what happens at which temperature increase)	Text on the link between RF and temperature will be added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2227	6	19	25	22	21	2) Is there ANY possibility to create and use CO2e stabilization pathways? Climate change is caused by all GHGs plus other climate forcers, so in a "simple" world you want to compare BAU and mitigation target in the same unit, namely CO2e. A use of CO2e stabilization pathways would also reduce communication complexity compared to a pure CO2 stabilization pathway (where you only can refer to CO2 emissions, and consequently different values)	We will aim to show CO2e pathways.
11418	6	19	26	20	1	The reference to the goal of international climate policy as defined in UNFCCC Art. 2 rewrites the treaty provision, is not complete, and therefore presents an incomplete and textually inaccurate picture of what this particular treaty provision actually provides. If reference to a treaty provision has to be done, it should be done faithfully and accurately. UNFCCC Art. 2 reads in full as follows: "The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time-frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner." Hence, the objective of the UNFCCC is not simply the stabilization of GHG concentrations at a level that would prevent dangerous anthropogenic interference with the climate system contained in the first sentence of the provision, but also that achieving such a level should be achieved in a way that also meets the objectives laid out in the second sentence of UNFCCC Art. 2. The second sentence is an important qualifier that has to be read integrally together with the first sentence in relation to how the stabilization goal is to be achieved. These two parts of UNFCCC Art. 2 cannot be separated from each other, whether conceptually or in practice.	Will consider to remove literal citation of UNFCCC.
6096	6	19	27	20	1	The text says "The goal of international climate policy as defined in UNFCCC art.2 is to stabilize greenhouse gas concentrations at a level that avoids dangerous anthropogenic interference of the climate system". This does not cover the whole meaning of the Article 2 of UNFCCC. Article 2 continues to say "Such a level should be achieved within a time-frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner". AR4 interpretes the article as "The criterion that relates to enabling economic development to proceed in a sustainable manner is a double-edged sword. Projected anthropogenic climate change appears likely to adversely affect sustainable development, with adverse effects tending to increase with higher levels of climate change and GHG concentrations. Conversely, costly mitigation measures could have adverse effects on economic development. This dilemma facing policymakers results in (a varying degree of) tension that is manifested in the debate over the scale of the interventions and the balance to be adopted between climate policy and economic development" (Chapter 1, p.99). The latter part of the Article 2 have something to do with Sections 6.3.3 and 6.3.4 of Chapter 6. Therefore, when discussing the goal of climate policy, the latter aspect should be definitely touched upon.	Will consider to remove literal citation of UNFCCC.
9857	6	19	9	19	12	As you state, structural changes can work in both directions, representing the aggregate level problem. You should further elaborate on this topic, as this is vital for the assumption and thus for the results of the model. How did you consider this issue in the report'?	Unfortunately there is not space to delve into a decomposition of the circumstances under which stuctural change increases or decreases energy service demand per unit output (nor is there typically sufficiently detailed reporting to do so across models).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6264	6	19		19		Given that this chapter is significantly over its alloted page limit, difficult choices are going to have to be made in terms of what to keep and to delete. Figure 6.6 (while a very nice and very informative graphic) might be a candidate for deletion as the previous graphic which uses the Kaya Identity speaks to these data. The text keeps making the point that the models used in this chapter are all different and produce different points. That is important and worth saying more than once. But perhaps this point is being repeated too much,i.e., keeping both Figure 6.5 and Figure 6.6.	The author team is considering dropping Figure 6.6 due to its limited added value to the chapter.
10985	6	19	10	19	12	Does this sentence mean that dispersed power sources such as PV develop while countries become wealthier and demand for energy-intensive services increase?	The example of shifts to less energy-intensive industries as countries become wealthier refers to the nature of economic activity: post-industrialized economies typically have a greater share of services (financial, legal, retail, etc.) relative to manufacturing (though every country's development path is different).
11746	6	20				If you distinguish how the table was developed in the next draft, remark should also be added that policy makers could understand the number of scenarios wasn't a matter.	We have added a note on the meaning of the number of scenarios
9413	6	20				How much did you count historical CO2 budget and non-CO2 budgets from 2000 to 2010? These amounts should be mentioned in a footnote of this table.	Will be done.
13759	6	20				What are the units?	unit will be added, thanks
6758	6	20				It shoud be specified that th e number of scenarios is not important. Cautions are required that the amount of the number of scenarios does not mean feasibility. □	We have added a note on the meaning of the number of scenarios
10646	6	20				It is necessary that IPCC put some remarks so that negotiators would not be misled by the big numbers of Cat. 1 and that they understant that the number is not a matter	We have added a note on the meaning of the number of scenarios
5858	6	20				Please do not forget to add units of measurement to the table and to explain what your definition of a "CO2 budget" is.	Units have been added, thanks
9983	6	20				This table should include an explanation in the footnote that each scenario and its result is only calculated example and the number of scenario itself is not important for appropriateness of a scenario. Interpretation of the same type of table articulated in the AR4 has been incorrectly recognized and misused.	We have added a note on the meaning of the number of scenarios
8041	6	20				When introducing the new RCP scenarios (RCP 2.6, 4.5, 6, 8.5) it is helpful for a lay reader to mention the 'rule od thumb' deviated from the climate sensitivity that a radiative forcing of 4 W/m2 leads to a 3 K warming. With that the reader can translate the abstract radiative forcing of a scenario to a temperature increase.	We have added a complete new section that links to temperature.
8042	6	20				Most of the data from table 6.2 are at least as instructive that those of table 6.3 which appears in the Ex Sum. as ES.1 . I suggest to export them to the Ex Summary, e.g. expand table ES.1	Many thanks. Will suggest to CLA
13134	6	20	11	20	11	I assume this is meant to say that there's no unique definition for representing concentration targets in the models? If correct please rephrase, if incorrect please explain.	We have added more text here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16355	6	20	27			I noticed the intention to "distinguish overshoot scenarios", which could go in a very useful direction. This table is very important due to its potential to provide a high profile synthesis. However, the scenario categories are currently very close to those from AR4, which ignored the potential for "overshoot scenarios" and failed to provide a sufficiently complete view of the potential for stabilisation. A lot of care will need to be taken to avoid oversimplifications that might be misleading (for example, how is it possible that there is only one value for the CO2 concentration in 2100 per category? to improve from AR4, it might be needed to take more data directly from WGI and/or from scenarios; uncertainty is another issue that needs attention)	It is very challenging to find a categorisation that captures all dimensions. We will do our best. The lack of uncertainty range to the CO2 numbers was an oversight.
7391	6	20	32	24	28	A key policy-relevant conclusion from these sections, currently missing, is that drivers for abatement of short- and long-lived GHGs are different. Policymakers need to understand to what extent transformation pathways are driven by physical earth-system properties, and to what extent by their own choices and more intermediate goals. Abatement of short-lived gases is driven primarily by economic efficiency and non-climate co-benefits (and perhaps a desire to limit rates of change), but is not absolutely necessary to achieve stabilisation (which currently is the only formally agreed long-term goal under the UNFCCC process). By contrast, abatement of long-lived gases to zero is an absolute must if stabilisation is to be achieved and only the pathway is driven by cost-effectiveness. Drawing this distinction out more clearly would be an important policy-relevant conclusion from these sections that affects the timing of peak emissions as well as overshoot as well as the balance of abatement between gases.	We will try to add some conclusions, without being policy descriptive. Problem is partly that there are very different views, even in science, on these issues.
7392	6	20	32	24	28	The discussion of the role of GHG metrics (GWPs, GTPs, optimisation etc) in these sections is incomplete. Recent work has shown (Reisinger et al 2012, accepted for Climatic Change; contact me for pre-print) that alternative metrics affect not only the allocation of mitigation across different gases, but also the timing of CO2 emission peaks and overshoot relative to a defined long-term goal. These findings, and the (relative un-)importance of metrics in a first-best policy context, should be reflected in this section as they address a key policy interest about metrics expressed by Parties to the UNFCCC. Also earlier studies that clearly demonstrate the impact of metrics on the timing of CH4 abatement (e.g. van Vuuren et al 2006) should be cited here. We know a lot more from the current literature about the influence of metrics on transformation pathways than is apparent from the current draft.	Text has been adapted and references added.
7679	6	20	4	20	4	Change "stabilization of greenhouse gases" -> "stabilization of greenhouse gase concentrations"?	Wording has been corrected.
7680	6	20	4	20	5	Change "there are types" -> "there are other types"?	Typo has been corrected.
6907	6	20	11	20	11	This statement is confusing -- "GHG concentrations" is clearly defined in, e.g., the Glossaries of all three WG contributions to AR5 (and in AR4, SREX, SRREN). I assume you meant to say that indiviudal models differ in the way which and how concentrations of GHG concentrations are being prescribed.	Text has been changed.
6908	6	20	17	20	18	Please also refer to WGI Chapter 7 regarding aerosol statement.	Reference has been added.
6909	6	20	22	20	25	Note that models run with an interactive carbon cycle and assessed in WGI AR5 will be run with prescribed emissions, calculating CO2 concentrations and radiative forcing interactively. Comparison with the numbers provided in Table 6.2 might thus no longer be straightforward. We also note that carbon cycle and carbon cycle climate feedbacks will result in a range of year 2100 CO2 concentrations (and radiative forcing values) for a particular scenario depending on model, climate sensitivity, carbon cycle setup etc. A lot of this information will be presented in WGI AR5 Chapter 6 (and 12). Please refer to these Chapters of WGI AR5.	Useful remarks. We will ensure consistency. The lack of ranges for the CO2 numbers was just an oversight.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6266	6	21		21		It is clear that a decision was made to use these labels "Category 1", "Category 2"… and that decision is unlikely to change at this point in the AR5 writing process. Please consider repeating (by for example inserting a column in Table 6.3) that repeats the information that translates Category 1 into an equivalent W/m^2 or ppmv everyonce in a while. It is too much to ask readers to continue to flip back to Table 6.2 throughout the course of this long chatpter. Since Figure 6.7 and Table 6.3 are on the same page it seems that repeating this information here in the Table would take care of the "translation" for both the graphic and the figure.	We will discuss how to best do this.
11419	6	21				There should be an explanation of why 2005 is selected as the emissions level base year rather than 1990, especially considering that 1990 is the base year that, up to now, has been agreed to at the multilateral policy level under the UNFCCC as the base year to which emissions levels would be compared.	We just chose the level used as base year in most model calculations to date. But we could add a footnote with some informations how to translate.
13136	6	21	17	22	1	There's something wrong with the sentence starting "Cumulative…". Neither table 6.3 nor figure 6.7 shows cumulative emissions either.	Text has been changed.
8105	6	21	18	22	1	Be aware that both Allen et al 2009. and Meinshausen et al. 2009 are discussing peak temperature targets, and not end-of-century targets as discussed in this section.	Correct. Will improve text.
8103	6	21	3	21	5	Specifying that here an overshoot of forcing is meant (and not necessary temperature) would be helpful to avoid misunderstanding.	Text was strongly rewritten.
14397	6	21	4			Negative emissions through BECCS is important, and could be mentioned earlier (especially if anything like the current language on zero emissions is kept). Need to clarify: CCS is by definition zero; it is the BE part that turns it negative. Memo: here, as several other places, the analysis cries out for more information on the likely prospects of CCS.	We make sure to improve the BECCS coverage
5232	6	21	4			Two different abbreviations are used for bioenergy CCS, namely bioCCS and BECCS. Please use just one.	We will choose BECCS.
8104	6	21	5	21	5	Insert "net negative emissions" in the long-term. As "negative emissions" are already assumed to materialize rather soon (in the next decades) in most scenarios with BECCS in the portfolio.	Correct. We have changed this.
9414	6	22				Findings are overlaping with Table6.2 and 6.3. To save space, these tables and figures can be incorporated	We are considering to merge these.
9858	6	22	10	22	12	This is crucial to the whole chapter: "Models differ" and reflects the assumption issue raised earlier. Please be more specific here to give the reader an insight into the model and especially stressing the intersubjectivity of the models.	We have tried to be transparent. However, there are severe page constraints.
8106	6	22	10	22	18	This paragraph can benefit from a statement explaining that, unless all scenarios were constructed for the under the same protocol, the wide ranges of the scenarios also depend on which question was analysed by the modeling teams.	We have added this.
9859	6	22	12			I don't see a discussion of the economic consequences of climate policy I section 6.2.4. What do you mean? You don't mean to refer to damage costs, do you?	Noted. This section was substantially rewritten, sentence is no longer contained.
8645	6	22	12			I don't see a discussion of the economic consequences of climate policy I section 6.2.4. What do you mean? You don't mean to refer to damage costs, do you?	Same comment as comment no 9859, see there for answer.
9860	6	22	19			The numbers for the cumulative carbon dioxide budgets allowed for Category 1 scenario at around 1400 GtCO2 (mid-range) seem too high, because that gives an average of about 28 GtCO2 per year for 2000-2005, or about today's level of emissions. Please check.	We will check - probably cause is negative emissions
8646	6	22	19			The numbers for the cumulative carbon dioxide budgets allowed for Category 1 scenario at around 1400 GtCO2 (mid-range) seem too high, because that gives an average of about 28 GtCO2 per year for 2000-2050, or about today's level of emissions. Please check.	We will check - probably cause is negative emissions
14454	6	22	20			Label graphs for clarity.	Done

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8107	6	22	22	23	3	Another reason is that for shorter-lived forcers (like methane) the rate of emissions at the time of maximum forcing/temperature is more important than the cumulative emissions over time. See Smith, S. M. et al. Equivalence of greenhouse-gas emissions for peak temperature limits. Nature Clim. Change 2, 535-538, doi:10.1038/nclimate1496 (2012).	Reference has been added.
13139	6	22	27	22	27	The reference should be to figure 6.9, I believe.	We will check.
13140	6	22	29	22	31	There's nothing about non-CO2 gases in section 6.2.1. Correct the reference.	We will check.
11420	6	22	3	22	3	There should be an explanation of why 2005 is selected as the emissions level base year rather than 1990, especially considering that 1990 is the base year that, up to now, has been agreed to at the multilateral policy level under the UNFCCC as the base year to which emissions levels would be compared.	2005 is the base year of most model runs.
13137	6	22	7	22	9	Does this mean that the baselines are not included in any of the categories? I would have thought that category 6, for example, would mostly have baseline scenarios in it (forcing being above 7 W/m2).	No they are discussed in the previous section.
13142	6	23	17	23	17	Write out GTP.	Will be done. Also reference to relevant places in Chapter 3.
5859	6	23	17	23	17	Please explain "GTP" and include the term in the glossary.	Will be done. Also reference to relevant places in Chapter 3.
10753	6	23	18	23	24	The possibility of a multi-gas policy that uses a multi-basket approach should be discussed (see last para of section 8.7.1.5 in WGI SOD).	We have one sentence now. Would need CA to take care of this.
8108	6	23	18	23	24	This paragraph would also need to acknowledge the publications that show that action short-lived pollutants might be detrimental or hamper CO2 abatement. For example see: Berntsen, T., Tanaka, K. & Fuglestvedt, J. Does black carbon abatement hamper CO 2 abatement? Climatic Change 103, 627-633, doi:10.1007/s10584-010-9941-3 (2010). AND Myhre, G., Fuglestvedt, J. S., Berntsen, T. K. & Lund, M. T. Mitigation of short-lived heating components may lead to unwanted long-term consequences. Atmospheric Environment 45, 6103-6106, doi:10.1016/j.atmosenv.2011.08.009 (2011).	We have added some text already.
4036	6	23	21			Missing reference - (UNEP and WMO, 2011). Should have been "UNEP and WMO (2001). Integarted Assessment of Black Carbon and Tropospheric Ozone. Available at http://www.unep.org/dewa/Portals/67/pdf/BlackCarbon_report.pdf	Corrected.
8109	6	23	27	24	2	Be aware that both papers discuss peak temperature targets, and not necessary the long-term impact. Therefore in case negative emissions do not scale up quickly enough so that they do not significantly influence the temperature peak, the findings of the above studies would still be valid.	Thanks you are correct.
9962	6	23	3		15	If CDR technologies and BECCS are main measures for negative emissions, maybe we have to introduce to what extent these measures should be implemented to reach negative emissions.	Text added.
8967	6	23	40		42	SRM by muliple actors may be more confusing than it is effective. I.e. what is background, what has been modified.	Noted. There is no reference to SRM on page 23, let alone lines 40 to 42. It is not clear to us what this statement refers to. We are sorry that we can not answer it for that reason.
10752	6	23	7	23	24	This para contains important information and could be expanded to assess what the effect would be of using a different metric than GWP. An assessment of how suitable the GWP100 is in a context of a stabilization goal would be useful (see WGI chapter 8 and references there).	No definitive conclusion is possible. But we provide some of the considerations.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10971	6	23	7	23	24	This paragraph covers GWPs and GTPs but does not seem to cover the point that neither of these is consistent with climate stabilisation. Shine et al 2007 is a very good paper. But an example showing that comparisons of CO2 and methane should be rather different for stabilisation is: Manning, M., and A. Reisinger, Broader perspectives for comparing different greenhouse gases Philosophical Transactions of the Royal Society A, 369, 1891-1905, 2011 - which brought Tom Wigley's Forcing Equivalence Index into the context of stabilisation. And then: Lauder, A., I.G. Enting, J.O. Carter, N. Clisby, A.L. Cowie, B.K. Henry, and M.R. Raupach, "Offsetting methane emissions --- an alternative to emission equivalence metrics", International Journal of Greenhouse Gas Control, (submitted), 2012 - have taken that further and shown that a one-off sequestration of CO2 can be a credible offset for continuous emissions of methane.	We have added some text.
10751	6	23	9	23	9	It should be made clear that this is GWPs for a 100 year time horizon.	Done.
13141	6	23	9	23	9	Write out GWP.	Done
6267	6	23	13	23	17	Here is another example of where there is a need for specific references to peer reviewed papers rather than an implicit reference to the database assembeled for Chapter 6 or the collective wisdom of the authors of Chapter 6 "There are also models that determine the relative reduction of different gases based on the overall cost optimization across time. If the latter approach is applied toward long-term radiative forcing goals, the emissions of short-lived gases tends to be postponed compared to models using GWPs."	Done. The text was deliberately formulated in a more generic sense. But we now added some examples.
6910	6	23	27	23	29	Please refer to WGI AR5 Chapter 12 which assesses the physical science basis of the emissions-CO2 concentration-radiative forcing-climate change relationship.	Done.
9415	6	24				Effects of BECCS in the latter half of the century have a large impact on emissions pathways in the first half of the century. This figure is the new finding since AR4 and very informative. However, it will be more informative if authors can add information how much BECCS are considered in each scenario or a range of BECCS among scenarios.	text has been added.
10393	6	24	10	24	11	The author may mistake Figure 6.10 for Figure 6.9.	Corrected.
9960	6	24	11		15	The sentence "Net negative emissions…..2050 emission reductions" is duplicated at the end of this paragraphy.	Corrected.
8043	6	24	3	24	15	BECCS is still highly speculative especially in the light of the negative development on CCS in many countries (see e.g. the projects the EU had planned and what is now). This should be reflected in this paragraph.	Agree. text has been added.
9099	6	24	39	24	39	"wastes" might be revised as "unused biomass"	text was rewritten.
13758	6	24	43	24	45	Does this refer to Fig. 6.11? Note that neither the figure nor the text are clear. I am confused. What do you mean? Is the objective of the climate model runs to end with a specific temperature or a specific concentration?	Paragraph was totally rewritten
6396	6	24	5	24	5	Bioenergy with CCS is referred to in at least three different ways in the chapter. Here it is BECS. On page 53, line 99 it is BioCCS.	We have replaced it with BECCS
14398	6	24	5			Is BECS different from BECCS?	We have replaced it with BECCS
8049	6	24	7	24	7	the reference should be (also) to chapter 6.9 which covers geoengineering	Yes. Reference is made.
2418	6	24	9	24	9	Note that SRM has not been introduced in the chapter yet at this point.	We will make sure that this is done.
10986	6	24	24	24	28	Fundamentally, CDR must be substantially one of the effective technologies for mitigation. Therefore, this paragraph should be deleted.	We do not agree. There is a difference in the way these technologies are covered in the current literature.
6911	6	24	6	24	7	Please refer to WGI AR5 Ch6 and 7 for the most up-to-date assessment of the physical science basis of CDR and SRM technologies.	We will do so.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6269	6	24	29	25	9	This section on Temperature Stabilization Scenarios feels out of place here. Can this be made into a footnote? This seems tangential to the core of Chapter 6.	We have shortened the text.
6912	6	24	29			Please refer to WGI AR5, Chapter 12.	We will do so.
4344	6	24	32	24	34	Response of temperature and CO2 concentration in climate system was investigated in detail and new concept of stabilization scenarios are proposed (Matsuno, Maruyama and Tsutsui, 2012a.b.) (see : https://www.jstage.jst.go.jp/browse/pjab) Papers are ; 2012a : T. MATSUNO, K. MARUYAMA and J. TSUTSUI ; Stabilization of atmospheric carbon dioxide via zero emissions—An alternative way to a stable global environment. Part 1: Examination of the traditional stabilization concept, Proceedings of the Japan Academy, Series B Vol. 88 No. 7, pp.368-384 2012b : T. MATSUNO, K. MARUYAMA and J. TSUTSUI; Stabilization of atmospheric carbon dioxide via zero emissions—An alternative way to a stable global environment. Part 2: A practical zero-emissions scenario, Proceedings of the Japan Academy, Series B Vol. 88 No. 7 pp.385-395.	References have been considered for reference.
10972	6	25	10	25	35	You will not be able to cover this in the chapter but it is time that some of us who know about atmospheric chemistry submitted a paper showing that reduction in the incoming solar radiation would reduce the hydroxyl radical which is not only doing more to reduce radiative forcing by all the greenhouse gases than the carbon cycle does - (as shown in Manning, M., and A. Reisinger, Broader perspectives for comparing different greenhouse gases Philosophical Transactions of the Royal Society A, 369, 1891-1905, 2011) but it is also the reason that we do not much higher levels of carbon monoxide and other toxic gases in the atmosphere. The idea sounds like a classic example of maladaptation where people focus on fixing just one problem and end up making the combination of all problems much worse.	Reviewer encouraged to submit paper.
13730	6	25	11	25	13	Rephrase " Another concept to affect climate variables such as temperature or precipitation is by directly altering radiative forcing (solar radiation management or SRM) for instance by adding aerosols at specific heights in the atmosphere to reflect a share of the incoming sunlight."	Thanks.
6397	6	25	13	25	13	I think this should reference Section 6.9, not 6.8.	Thanks.
13731	6	25	15	25	18	Rephrase "The predominant reason is due to the different decision rationale: SRM requiring a risk-balancing approach, whereas mitigation strategies share similar levels of risk and are thus addressed by the cost-effectiveness rationale that currently forms the focus of most IAM analysis (Barrett, 2008)."	We will consider this wording.
13143	6	25	18	25	18	This reference is not in the bibliography.	Will be added
8111	6	25	20	25	22	Although not necessarily wrong, this statement is not very relevant in view of what was written earlier in this chapter. On Page 19, line 27, which refers to article 2 of the UNFCCC. Firstly, the convention mentions concentrations to be stabilized, not necessary forcing. Secondly, it also aims at avoiding dangerous anthropogenic interference with the climate system. In line with the latest results from the earth system modelling community with regard to solar-radiation management, injection of stratospheric aerosols can in itself be considered dangerous anthropogenic interference with the climate system, and as such be ruled out as a mitigation option. See: Ricke, K. L., Morgan, M. G. & Allen, M. R. Regional climate response to solar-radiation management. Nature Geosci 3, 537-541, doi:http://www.nature.com/ngeo/journal/v3/n8/suppinfo/ngeo915_S1.html (2010). AND Schmidt, H. et al. Solar irradiance reduction to counteract radiative forcing from a quadrupling of CO2: climate responses simulated by four earth system models. Earth Syst. Dynam. 3, 63-78, doi:10.5194/esd-3-63-2012 (2012).	We will consider the article. We will obviously also ensure that the discussion on SRM is balanced. Finally, it is questionable whether the objective of UNFCCC was written in a way to rule out SRM; probably avoiding dangerous climate change is the key part of the objective. In Chapter 3 there is a more elaborate discussion on the priciples related to SRM/Geoengineering.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8112	6	25	20	25	22	Although not necessarily wrong, this statement is not very relevant in view of what was written earlier in this chapter. On Page 19, line 27, which refers to article 2 of the UNFCCC. The "climate system" under the UNFCCC is defined as (see article 1, definitions) "the totality of the atmosphere, hydrosphere, biosphere and geosphere and their interactions.". Currently global warming is at the core of the debate (and this could theoretically be coutnered by solar-radiation management), but other dangerous anthropogenic interferences because of rising greenhouse gas concentrations are not excluded. For example, ocean acidification (part of the hydrosphere and with strong impacts on the biosphere), will not be halted with solar-radiation management which reduces forcing but not greenhouse gas concentrations. A statement higlighting this might be useful.	We will obviously also ensure that the discussion on SRM is balanced. Finally, it is questionable whether the objective of UNFCCC was written in a way to rule out SRM; probably avoiding dangerous climate change is the key part of the objective. In Chapter 3 there is a more elaborate discussion on the priciples related to SRM/Geoengineering.
13732	6	25	23	25	35	Political moral hazard and lock-in need to be mentioned here! Also the inherent threat of SRM due to the impossibility to test it on significant scales other than actually deploying it! The following is an attempt to bring this consideration to the point. Rephrase line 30 "This attribute of SRM makes it attractive to the present day decision maker in managing climate risk even if the long-run costs and damages of SRM were comparable to the costs of mitigation and the damages of climate change (Moreno-Cruz and Keith, 2012). At the same time though the possibility to avoid investments in near term emission reductions create a strong incentive to deploy SRM without investing in the long-term optimal level emission reductions. This incentive structure could prevail over decades resulting in both increasing GHG levels and a need for further SRM efforts (Klepper et al., 2012)."	We agree that some mention of the risk of policy and technical lock-in is important. The statement that it is impossible to test SRM at sales short of deployment is false and not backed up by the literature. The proposed revised text assumes that optimal policy somehow should not reduce emissions in mitigation if SRM was feasible, this is not backed up by literature.
2420	6	25	26	25	26	"Absent SRM" should read "In the absence of SRM"	The usage is correct (though uncommon) as written.
8971	6	25	30		31	There is no attribute of SRM that is "valuable." It is a thoroughly untested prospect.	SRM may work less well than studies now suggest or may have larger risks but the statement that there is no aspect of it that is “valuable” seems hard to defend given that (to our knowledge) all studies that actually looked at climate damages suggest that SRM could reduce them.
8044	6	25	31	25	34	The idea 'SRM cannot precisely counteract the radiative forcing from greenhouse gases' should come at the beginning and not at the end.	Yes, good suggestion.
16697	6	25	38			Define RCP.	Will be defined earlier in the chapter
9861	6	25	40			Please briefly explain the relevant differences between IAM models and the "complex climate models" refered to here. In what ways are the complex climate models better?	We will avoid the word complex models. We will refer to WG-1 model. If space allows we add some further explanation.
8647	6	25	40			Please briefly explain the relevant differences between IAM models and the "complex climate models" refered to here. In what ways are the complex climate models better?	We will avoid the word complex models. We will refer to WG-1 model. If space allows we add some further explanation.
13144	6	25	43	25	43	Figure 6.11, instead of 6.8	Thanks
10394	6	25	43	25	43	The same problems as the fomer. It's Figure 6.11 not figure 6.8.	Thanks

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9964	6	25	43			"Figure 6.8" should be "Figure 6.11".	Thanks
13145	6	25	46	25	47	Is there a reference for this analysis?	Noted. This section was completely rewritten in the new draft.
7681	6	25	8	25	9	The section ends with "[…] temperature target needs to be expressed in terms of a probability […]". Although the cited papers might emphasize such, it is not generally true. The emission pathway doesn't have to be decided at one instant, as e.g. most IAM's assume. We can observe the temperature increase later during the century, and adjust the emission pathway recurrently so that the temperature target will be ultimately met. Scenarios with a temperature target and risk-hedging through sequential decision making include: * Syri, S., Lehtilä, A., Ekholm, T., Savolainen, I., Holttinen, H. & Peltola, E. (2008), 'Global energy and emissions scenarios for effective climate change mitigation - deterministic and stochastic scenarios with the TIAM model', International Journal of Greenhouse Gas Control 2(2), 274–285. * Webster, M., Jakobovits, L. & Norton, J. (2008), 'Learning about climate change and implications for near-term policy', Climatic Change 89(1-2), 67–85. * Johansson, D. J. A., Persson, U. M. & Azar, C. (2008), 'Uncertainty and learning: Implications for the trade-off between short-lived and long-lived greenhouse gases', Climatic Change 88(3-4), 293–308. * Ekholm, T. (submitted), Hedging the climate sensitivity risks of a temperature target. Submitted to Resource and Energy Economics in Feb. 2012.	This is to some degree correct and we will make sure that we refer better to sequential decision making literature (this is done already further in the Chapter). Nevertheless, to get an idea of the overall size of the problem NOW it matters a lot what probability we are thinking of. Moreover, inertia will always imply some interpretation.
2419	6	25	10	25	35	This section should cross-reference Chapter 7 of WG1 assessment, which deals specifically with the physical aspects of SRM. The reference to section 6.8 at the end of the section should be 6.9 in fact.	Yes, good suggestion.
2422	6	25	10	25	35	A key aspect of SRM is that it has to carry on until the concentrations of greenhouse gases have gone down (either because of natural sinks or artificial CDR) or until society can better adapt to a warmer climate. This characteristics of SRM, usually referred to as the "termination issue", ought to be discussed in the context of stabilisation scenarios.	We know of no published argument that shows why SRM "must" be continued until concentrations of greenhouse gases have gone down. It is widely understood that many climate impacts depend on the rate of change of climate. SRM could be used to reduce the rate of change without being continued until concentrations are reduced.
6268	6	25	10	25	35	Delete Section 6.3.2.6 as a standalone section and fold any key points into the previous page's discussion of CDR. At this early point in the chapter the key points that need to be made are CDR and SRM might open up the policy space, are not represented in IAM models and will be discussed later. That can be combined into a couple of sections. This chapter is too long as it is. Here is a plce to cut back and save half a page.	Might make sense.
6913	6	25	13	25	13	Please refer to WGI AR5, Chapter 7.	Will be done
4196	6	25	25			Reference is insuffficient. Sprecial Report of IPCC and others should be quoted.	Will be done
6270	6	25	38	25	47	I do not think that "complex models" is a generaly used scientific term. If these "complex models" are General Circulation Models, please use that term. I don't think the reader is helped by creating a new term "complex models."	Agree. We change the wording.
6914	6	25	41	25	43	We suppose this should be turned around -- the IAMs are consistent with the results from complex climate models for, e.g., temperature etc. and not vice versa.	Agree. We change the wording.
11265	6	26				if temperature profiles from WG1 could be added here this would be very useful	We will add a totally new section on temperature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
18632	6	26				Page 26: No cost-benefit study finds an optimal level of mitigation that stabilizes tha atmospheric concnetrations I question if decarbonisation really is an optimisation problem	Noted
6915	6	26				Please provide source of this figure.	Figure will be removed
10973	6	26	12	26	24	This is a good paragraph and helps to offset some of the problems in chapter 3's appearing to over-emphasise the relevance of discount rates.	Noted
9862	6	26	15	26	16	Climate feedbacks don`t occur on the individual company level directly, i.e. companies might have the possibility to manage their CO2-emissions, but are not impacted by climate change and vice versa. The causal link on the company level is via external effects.	Noted
14399	6	26	16	26	21	But cost-benefit models do show the extra cost of stabilization at a given target level, above and beyond what their optimal level is, and there is very important information in the fact that the additional cost tends to be small.	Noted
6097	6	26	16	26	30	The text says because, in all cost benefit analysis, concentrations continue to rise throughout the modeling period, studies that focus on cost-benefit are not appropriate for the discussion of transformation pathways in this chapter. If this is the real reason of not taking up CBA in this chapter, then, what can be done in this chapter is to show cost and benefit for typical (given) transformation pathways. You can find this kind of CBA, for example, in many books and papers of Nordhaus where Nordhaus calculated cost and benefit for several given targets, such as limiting temperature increase by 2 degrees. As a matter of fact, the text in this chapter describes as "mitigation, impacts and adaptation are interlinked in several important ways and should be considered jointly in the context of achieving stabilization targets (page 26, lines 27-29). I am not quite sure what the relationship between this sentence and CBA.	Taken into acccount--text added and reference given to other parts of the report that discuss CBA
8648	6	26	18			The logical conclusion that one would draw from the statement that begins "no cost benefit study finds an optimal level of mitigation…" is that the damage costs used in such studies are probably too low, because it seems non-sensical to allow greenhouse gas concentrations to rise until at least 2100. If this is true, please say so. In fact, while this section does not discuss the reasonableness of the damage cost estimates used in such modeling exercises, again a naive reader might think that it is impossible to estimate the damages from climate change since the natural and human systems impacted are far too complex. Thus, I think it would reasonable to have a short critique of the reasonableness of damage cost estimates here, if you are going to introduce the topic of cost-benefit studies at all. The point that these studies are not relevant when developing stabilization scenarios as with using the RCP methodology is a good one, and should be highlighted more.	Taken into account--but due to space constraints, we are unable to add a discussion of damage function estimates. We have added references to chapters 2 and 3 which provide a larger discussion of CBA.
10395	6	26	18	26	19	There are some models can find an optimal level of mitigation as while as stabilizing atmospheric. Such as RICE and MRICE model etc.	Taken into account--text added
6098	6	26	31	27	4	It is hard to understand how climate impacts, mitigation and adaptation responses in integrated assessment are meaningfully interlinked without knowing cost of impact and adaptation. Please rewrite.	Taken into account--figure and text deleted
8972	6	26	35			Geoengineering is not a form of adaptation! It is intervention. So please delete such language.	Taken into account--figure and text deleted
8507	6	26	35	27	1	It is impossible “to decouple GHG concentrations from climate variables such as temperature”. It is possible to compensate the GHG effect on temperature.	Taken into account--figure and text deleted
15428	6	26	35	27	1	DELETE: "(a form of adaptation)" This cannot remain in the text because there is no agreement within the IPCC that geoengineering is a form of adaptation. Further, there must be acknowledgment of the ambiguous, controversial and speculative nature of geoengineering. To include this parenthetical statement obscures and minimizes -- in fact, denies -- the ambiguity, controversy and speculation surrounding geoengineering.	Taken into account--figure and text deleted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11421	6	26	8	26	10	It is good that this sentence is present, recognizing that most mitigation pathway studies typically do not factor in climate impacts and adaptation. This is the reason why having a correct and accurate reflection of UNFCCC Art. 2 is important - doing so allows, and in fact would encourage, having a more holistic and scientific approach to mitigation pathway studies by requiring the integration of other factors such as climate impacts, adaptation measures, etc on human and natural systems that could have synergistic effects on various mitigation pathways and scenarios.	Noted
6271	6	26	18	26	22	Again, I think you need to identify these studies on cost benefit that are said to not be appropriate for the analysis in Chapter 6. Need to cite these papers. Cant just assert that these papers exist and they are not appropriate for what is discussed in this chapter.	Accepted--citations added
4307	6	26	35	26	35	delete „(a form of adaptation)“ because there is no consensus that geoengineering is either mitigation or adaptation. moreover, some actors (BMBF 2011) argue for a „MAG-approach“, seein geoengineering as the third leg of climate policy	Taken into account--figure and text deleted
6272	6	26	5	28	26	Because of how much this chapter is already over its alloted page limit, serious consisderation should be given to radically reducing the length of section 6.3.3. The key point is that IAMs (which are the heart of what Chapter 6 is about) do not handle adaptive responses very well. That point is made over and over and over again. Figure 6.12 is not needed and there is no need to introduce the terminology about adaptive responses Type 1, Type 2 and Type 3. Just make the key points and move on. This might be important research but it doesn't need to be treated at this level of detail in an already over page lenght limit Chapter 6.	Taken into account--figure and text deleted
14335	6	27				This figure and the corresponding text are not clear on how to classify geoengineering technologies, i.e. as mitigation, adaptation or something else (cf also chapter 1 page 25 line 39-40). The text should make clear that this is an unresolved issue in the geoengineering debate. The classification as mitigation, adaptation or something else can matter, in particular in a normative context such as the UNFCCC (Bodle, Ralph, “Climate and Geoengineering”, in: Hollo, Erkki, Kati Kulovesi and Michael Mehling (eds.), Climate Change and the Law: A Global Perspective, Berlin: Springer, forthcoming 2012 (submitted May 2012), section 3.5).	Taken into account--figure and text deleted. Geoengineering now discussed elsewhere (6.3.3.2)
4197	6	27	18			Ciscar et al (2011) does not appear in the reference.	Accepted--reference added
9864	6	27	25			Again, I would emphasize the point much more strongly that "there is a desperate lack of data…." In particular, as far as I know there is absolutely no empirical basis for the type of damage cost functions that models like DICE include in them.	Noted
8649	6	27	25			Again, I would emphasize the point much more strongly that "there is a desperate lack of data…." In particular, as far as I know there is absolutely no empirical basis for the type of damage cost functions that models like DICE include in them.	Noted
6399	6	27	26	27	26	can remove a fair amount of discussion of what does not exist. Also, "necessitates heroic efforts" is colloquial. Suggest changing to something like "requires a lot of effort"	Editorial
9416	6	27	28			This flow-chart and its explanation is not easy to understand	Taken into account--figure and text deleted
5860	6	27	30	27	32	This is a careless use of the word "heroic" for a work that is - although not easy and not done within hours - business as usual for a lot of modellers: brigding gaps in data sources and correlations. Please delete.	Editorial
13733	6	27	33	27	36	Rephrase "As represented by the blue dashed lines in Figure 6.12, these strategies and responses compete for political attention, investment and R&D resources, leading to potential trade-offs as discussed further below. Also, as captured by the red dashed lines, physical feedbacks will affect the set of available mitigation and adaptation options, ..."	Taken into account--figure and text deleted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15429	6	27	33		34	Again, this sentence explicitly declares (and the accompanying Figure 6.12 suggests) geoengineering to be a type (one of three types) of adaptation -- this is unjustified. Further, there was NO AGREEMENT on this point at the Joint Expert Meeting on geoengineering held in Lima in June 2011. According to the Meeting Report, SRM "does not fall within the usual definitions of mitigation and adaptation." (IPCC, IPCC Expert Meeting on Geoengineering, Meeting Report, p. 2.)	Taken into account--figure and text deleted
13734	6	27	36	27	36	Insert "Some geoengineering strategies (solar radiation management approaches) attempt to decouple GHG concentrations from climate variables, …"	Taken into account--figure and text deleted. Geoengineering now discussed elsewhere (6.3.3.2)
9866	6	27	37	28	26	For decision makers the conclusion of these very important paragraphs could be that finding a solution is very complex and might even lead into the wrong direction. So I would add a paragraph how these biases can be overcome. Applying scenario thinking and the scenario technique would be an appropriate tool for organizations.	Taken into account--space constraints limit our ability to elaborate on this point
11422	6	27	37	28	26	This paragraph should be further expanded to explain the ways in which omission of climate impacts and adaptation responses could skew and provide inaccurate projections with respect to mitigation pathways. The authors should make a greater effort to search for literature - perhaps from non-OECD countries - that could provide approaches or ways in which such factors could be reflected in mitigation projections and scenarios.	Taken into account--space constraints limit our ability to elaborate on this point
9865	6	27	38			I think first you should say in simple English - including climate impacts will most likely show that the state of the world will be much worse than IAMs indicate, and, therefore, even more climate mitigation will likely be required to achieve any particular stabilization goal. For example, one type of negative feedback loop you don't mention is the one where higher global temperatures melt the tundra releasing additional quantities of methane into the air. Your first point is too specific and detailed. I think there are much broader possible implications that should be mentioned even if there are no good modeling exercises supporting such implications.	Taken into account--space constraints limit our ability to elaborate on this point
8650	6	27	38			I think first you should say in simple English - including climate impacts will most likely show that the state of the world will be much worse than IAMs indicate, and, therefore, even more climate mitigation will likely be required to achieve any particular stabilization goal. For example, one type of negative feedback loop you don't mention is the one where higher global temperatures melt the tundra releasing additional quantities of methane into the air. Your first point is too specific and detailed. I think there are much broader possible implications that should be mentioned even if there are no good modeling exercises supporting such implications.	Taken into account--space constraints limit our ability to elaborate on this point
11747	6	27	39	27	41	If such significant water shortage is occuer, not only nuclear and hydro but also thermal powers are affected that might force industry sectors to ristrict their production. This instance is so extreme that it should be deleted.	Accepted---text revised
9568	6	27	39	27	41	Please, reflect the following fact; in some regions such as Japan, seawater is required for thermal cooling for nuclear power plants.	Accepted---text revised
9091	6	27	39	27	41	Which countries will be suffering from water shortage for cooling nuclear reactor ? Appropriate references should be shown to validate this sentence.	Taken into acccount--text revised
12018	6	27	39	27	41	Regional water shortage may be severe and may affect particular site of any facilities which need water for cooling. However, it is misleading to generalize the statement without quantitative analysis. Some reservois may benefit from more rains. Facilities which use sea water for cooling are unlikely to be affected much. Balanced statement should be needed.	Taken into account--text revised
11748	6	27	41	27	42	The words [Also,] and [anothoer] aren't needed. Refer to No.33.	Editorial
13735	6	27	41	27	41	Use term "low-carbon" (instead of carbon-free)	Accepted
5861	6	27	41	27	41	Again: "there is no free lunch" and no such thing as a "carbon-free energy source" if there have been and are C emissions during facility construction, maintenance and operation.	Accepted--text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13736	6	27	42	27	42	Use "affect" (instead of negatively impact) ... another source of low-carbon energy (instead of carbon-free).	Editorial
8045	6	27	43	27	46	The intelligence in 'Unfortunately, there are no published modeling studies that account for the effects of climate impacts and adaptation responses on the set of viable mitigation strategies to reach stabilization targets' is very important and is worth to be mentioned prominently in the Exec. Summary. Same for 'Therefore, there is little information by which to judge how the omission of impacts and adaptation responses would alter the results reviewed in this chapter.'	Noted
9863	6	27	5	27	10	Is their an empirical study that comes to this conclusion? How are the three types differentiated?	Taken into account--figure and text deleted
6398	6	27	9	27	9	"further down the chain" is colloquial. Suggest changing to "that propogate through the system" or something like that.	Taken into account--figure and text deleted
10987	6	27	38	27	41	The sentence of "water required for thermal cooling in the case of nuclear power and stream flow required for hydroelectric power could face severe shortages as a result of climate change" seems to be a little exaggerated. It is logically leaping that mitigation option by using nuclear power or hydro would be restricted due to the assumption mentioned above. For example, nuclear power stations in countries like Japan are basically built along seaside; therefore, there are no worries about the scarcity of water.	Taken into account--text revised
4772	6	27	39	27	41	The statement "For instance, water required for thermal cooling in the case of nuclear power and stream flow required for hydroelectric power could face severe shortages as a result of climate change" is true. However it could be interesting to note that storage provided by reservoirs associated to dams are very important for climate change adaptation.	Noted
5862	6	28				Is there no relation of GE effects and Investment & R&D resources? Or does the figure - which lacks a legend, by the way - only show influences on and by the distribution of I & R&D-Resources?	Taken into account--figure and text deleted
6099	6	28	1			Interlinkage of the text itself (from lines 5-32, page 27) and Figure 6.12 is not so clear to at least ordinary readers. Much more simple way of explanation without this Figure could be possible.	Taken into account--figure and text deleted
8651	6	28	12			I would not say that the implications for transformation pathways are ambiguous. Most feedback loops due to climate change impacts are negative, as far as I know. (Are there any positive ones of significant magnitude?) Therefore, the implications for transformation pathways are highly negative.	Accepted--text revised
13738	6	28	14	28	15	Rephrase "Finally, mitigation strategies will need to compete with adaptation and possibly even SRM strategies for political attention, scarce investment and R&D resources. In accordance to the considerations in …"	Noted--will be considered in revision
8652	6	28	14			The paragraph that begins on line 14 I would suggest omiting. It goes too far in the direction of describing work that has never been done at all properly.	Noted--will be considered in revision
10974	6	28	2			Something that is not covered in a figure like this is the response time for the connections. Emissions affect concentrations directly, but there is a lag of decades for change in the climate variables, in many cases we find another lag of 20 years or so for development of defense to things like increasing flood risk, and at the same time there is expected to be a delay of several decades in the deployment of mitigation technologies. I think that the Fisher-Vanden report is a good one, but it admits that it is not covering everything. So if you are modifying the figure can you say something about lag times.	Taken into account--figure and text deleted
14037	6	28	4		13	Potential conflicts between stabilization goals and adaptation goals are in fact very much the case when local plans are made. One example is the densely populated city vs a city that has a natural buffer against, for example, extreme heat or extreme rainfall.	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4308	6	28				Table 6.12. implies a somewhat natural order of counter measures (mitigation - geoengineering - adaptation). Other sources see geoengineering more as a third way apart from the two „traditional“ ones. (see Rickels, W.; Klepper, G.; Dovern, J.; Betz, G.; Brachatzek, N.; Cacean, S.; Güssow, K.; Heintzenberg J.; Hiller, S.; Hoose, C.; Leisner, T.; Oschlies, A.; Platt, U.; Proelß, A.; Renn, O.; Schäfer, S.; Zürn M. (2011): Gezielte Eingriffe in das Klima? Eine Bestandsaufnahme der Debatte zu Climate Engineering. Sondierungsstudie für das Bundesministerium für Bildung und Forschung.)	Taken into account--figure and text deleted
6273	6	28	14	28	21	I'm not sure I am willing to agree that society's response to climate change is a strict zero sum game. This might be the way it has been modeled in the studies cited here but that does not mean that is the way this has to play out.	Noted
10988	6	28	21	28	26	Examples of "other expenditures" should be illustrated in order to grasp how they are important in estimating the "actual" economic cost of climate damages. Without knowing them we cannot assume the real scale of crowding out expenditures.	Noted--will be considered in revision
2228	6	29	1	33	14	ESSENTIAL: The IPCC should consider to contrast the sum of three cost elements to society when presenting this UNDER DIFFERENT SCENARIOS: 1) Mitigation, 2) Adaptation, 3) Damage cost. Then it will get obvious, that with more money invested in mitigation the TOTAL cost to society can actually be kept lowest. (Dentist analogy). The current representation of JUST showing cost for mitigation only, has of course the consequence that the more mitigation you are doing, the more cost you will incur. Consequently, mitigation is seen as MAIN cost to society, while the other cost elements will likely be bigger and will have much higher uncertainty.	The IPCC discusses the costs of climate impacts and adaptation in WG2 reports and the costs of climate mitigation in WG3 reports. Cost benefit analysis integrating costs and benefits of climate policy is explicitly discussed in the WG3 report, in Chapter 3 and Section 6.3.3 of Chapter 6. Section 6.3.4 focuses on mitigation costs. The introduction provides some context on climate benefits and co-benefits of climate action.
2229	6	29	1	33	14	Rethink the graphs in this section (6.13, 6.14, 6.15) - very hard to understand and interpret. Are consumption losses and abatement costs additive? At minimum have a concise description in the text WHY you chose those metrics and elaborate HOW those are calculated (e.g. example), i.e. define the metrics	Taken into account - Figures revised
6400	6	29	12	29	17	These points seem to be reinforced earlier in the chapter. Might want to consider removing or condensing here.	The paragraph has to keep a balance between providing the necessary context for the mitigation cost results and avoiding a general discussion of cost measures provided already in Chapter 3 (see Comment 516 which asks for expanding the material)
9870	6	29	12			This paragraph starting on line 12 has some good discussion of other factors to consider other than costs OR BENEFITS. But I would also add that more consumption should not always be taken as a "good". It depends on what is being consumed. Consuming more military weapons, or coal-fired power plants, or doctors visits are not good, if the result is more war, climate change, or sickness. You should acknowledge, therefore, that many people are seriously questioning whether or not GDP is at all a good measure of social benefits (see for example the Sen, Stiglitz, Fitoussi Commission in France oder the UN Human Development Index). Other indicators of social progress are being proposed frequently now. These comments also apply especially to the discussion from lines 29 to 45 below.	The paragraph has to keep a balance between providing the necessary context for the mitigation cost results and avoiding a general discussion of cost measures to be provided in Chapter 3 (see Comment 515 which asks for condensing the material)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8656	6	29	12			This paragraph starting on line 12 has some good discussion of othe factors to consider other than costs OR BENEFITS. But I would also add that more consumption should not always be taken as a "good". It depends on what is being consumed. Consuming more military weapons, or coal-fired power plants, or doctors visits are not good, if the result is more war, climate change, or sickness. You should acknowledge, therefore, that many people are seriously questioning whether or not GDP is at all a good measure of social benefits. Other indicators of social progress are being proposed frequently now. These comments also apply especially to the discussion from lines 29 to 45 below.	The paragraph has to keep a balance between providing the necessary context for the mitigation cost results and avoiding a general discussion of cost measures to be provided in Chapter 3 (see Comment 515 which asks for condensing the material)
16700	6	29	15			insert after 1st word "things," the following "costs of climate change impacts"	Rejected. Costs of climate change impacts are highlighted at the end of the paragraph.
14038	6	29	15			Add equity concerns	The paragraph has to keep a balance between providing the necessary context for the mitigation cost results and avoiding a general discussion of cost measures to be provided in Chapter 3 (see Comment 515 which asks for condensing the material)
9569	6	29	17			Please, delete examples of nuclear and CCS, or add examples of wind power and geothermal as they involve bird strikes (wind power) and sources of mercury contamination (geothermal power).	Taken into account - Examples removed
9570	6	29	17			Please, remove coal-fired from coal-fired CCS as we need any types of CCS.	Taken into account - Removed
4198	6	29	18	29	19	Some ancillary benefit and that of market reform could be assessed by CGE as discussed in TAR-WG3.	Noted
16701	6	29	22		24	The sentence "Reduced or negative mitigation costs …" I have not seen credible economic analysis that clearly shows these are very large or indeed real. Engineering potential is not economic potential is not market potential. "Negative mitigation costs" may simply not be accounting for additional costs that are all to real for investors or consumers.	Partly taken into account - The reference to negative mitigation costs is removed. Some models show a significant reduction of mitigation costs due to, e.g., revenue recycling from a carbon tax (Waisman et al., 2012, Climatic Change 11(1))
9867	6	29	4			This is true by definition of a mitigation scenario, correct? If so, say so or omit, it sounds a lot like a tautology.	Taken into account - rephrased
16698	6	29	4		11	It is very important to contextualize the economic impact as a slight reduction in economic growth rates from the no policy scenario -- many readers assume this is a reduction in GDP from today's level, not knowing that the model still shows continued economic growth. Perhaps one way to help the reader is to translate the "loss" or "cost" into a time lag until the same level of GDP or welfare is achieved in the no policy scenario.	Taken into accout - Presentation of costs will include this consideration
8653	6	29	4			This is true by definition of a mitigation scenario, correct? If so, say so.	Taken into account - rephrased

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9871	6	29	46			Again, it is not clear that "costs represented by the area under the margiinal abatement cost function" are at all relevant to computing the AVERAGE net costs of mitigation, which seems to me to be the needed calculation. Do you really mean "marginal" costs? Either way, please provide a graphic to illustrate this calculation. Is the other axis of the graph the amount of mitigation in tons of CO2? Explain clearly how incremental mitigation costs between two scenarios can be computed from marginal costs. This seems to be a conceptual error because marginal costs will change significantly over the range of mitigation of greenhouse gases required to go from a baseline case to an scenario like the RCP2.6 scenarios. In fact, marginal costs can be negative for at least some of the first tons of mitigation.	Rejected. The mitigation costs from partial equilibrium models reported here refer to the area under the marginal abatememt cost curve (reconstruced as a diagnostic device from the model, thus including full path dependency), and therefore are full costs. Explanation in the introduction has been clarified. Adding a figure is not possible due to space constraints.
8657	6	29	46			Again, it is not clear that "costs represented by the area under the margiinal abatement cost function" are at all relevant to computing the AVERAGE net costs of mitigation, which seems to me to be the needed calculation. Do you really mean "marginal" costs? Either way, please provide a graphic to illustrate this calculation. Is the other axis of the graph the amount of mitigation in tons of CO2? Explain clearly how incremental mitigation costs between two scenarios can be computed from marginal costs. This seems to be a conceptual error because marginal costs will change significantly over the range of mitigation of greenhouse gases required to go from a baseline case to an scenario like the RCP2.6 scenarios. In fact, marginal costs can be negative for at least some of the first tons of mitigation.	Rejected. The mitigation costs from partial equilibrium models reported here refer to the area under the marginal abatememt cost curve (reconstruced as a diagnostic device from the model, thus including full path dependency), and therefore are full costs. Explanation in the introduction has been clarified. Adding a figure is not possible due to space constraints.
9868	6	29	6			Again, as I stated earlier, both behavioral changes and the use of emissions mitigation technologies can lead to economic BENEFITS as well as costs to both producers and consumers. Where the net cost or benefit comes out in each year of each scenario is complicated. The text has this one-sided bias towards always talking about costs.	Rejected. Costs are put into context in the introduction, referencing the appropriate place where direct benefits from reduced climate change (WG2) amd co-benefits (Section 6.6) are discussed. This section is about mitigation costs.
16699	6	29	6			Insert the word "gross" before "economic costs". These costs are not net of any benefits or avoided losses from mitigation. While we have not tried to quantify these benefits, it never hurts to remind policymakers or the casual reader that benefits are created -- talking costs only neglects the value gained by action -- this makes it much harder for a policymaker.	Taken into account - Inserted
8654	6	29	6			Again, as I stated earlier, both behavioral changes and the use of emissions mitigation technologies can lead to economic BENEFITS as well as costs to both producers and consumers. Where the net cost or benefit comes out in each year of each scenario is complicated. The text has this one-sided bias towards always talking about costs.	Rejected. Costs are put into context in the introduction, referencing the appropriate place where direct benefits from reduced climate change (WG2) amd co-benefits (Section 6.6) are discussed. This section is about mitigation costs.
9869	6	29	8			It is not "common" to estimate the incremental costs of mitigation against a counterfactual base case, you must by definition of "incremental". Please clarify this sentence.	Rejected. Incremental is not used in the sentence.
8655	6	29	8			It is not only "common" to estimate the incremental costs of mitigation against a counterfactual base case, you must by definition of "incremental". Please clarify this sentence.	Rejected. Incremental is not used in the sentence.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
18633	6	30				Page 30: 5 % is choosen as the discount rate used to calculate the net present value. What are the consequences? (and to what extent is it relevant)	Taken into account. Information on the sensitivity of costs to choice of discount rate is now provided.
16702	6	30	1			Insert 'but deficient' after the word "popular".	Taken into account. If sentence is retained after shortening the discussion, deficient will be inserted.
9873	6	30	20			Here it says that transformation pathways have been derived under a range of discount rates. Then you say that you will consistently translate reported time aggregate costs into a total consistently using a 5% discount rate. But even if you have the annual cost results in current dollars for every scenario reported (do you?), the pathway is computed using the original discount rate, so there would be an inconsistency in reporting the results of an optimization using one discount rate in discounted dollars using a different discount rate, no? Moreover sensitivity analyses should be provided to show the impact of different discount rates on the results.	Taken into account. Yes, we have the annual cost for every scenario in the database. Information on the sensitivity of costs to choice of discount rate is now provided.
8659	6	30	20			Here it says that transformation pathways have been derived under a range of discount rates. Then you say that you will consistently translate reported time aggregate costs into a total consistently using a 5% discount rate. But even if you have the annual cost results in current dollars for every scenario reported (do you?), the pathway is computed using the original discount rate, so there would be an inconsistency in reporting the results of an optimization using one discount rate in discounted dollars using a different discount rate, no?	Taken into account. Yes, we have the annual cost for every scenario in the database. Information on the sensitivity of costs to choice of discount rate is now provided.
9966	6	30	22			Please make a reference to Table 3.1. It's helpful to know different values on discount rate.	Taken into account. Information on the sensitivity of costs to choice of discount rate is now provided.
4199	6	30	23			When GDP is discussed as a cost indicator, the issue about GDP in current price, GDP in constant price and GDP-PPP should be touched upon, in my view.	Rejected. GDP losses are not discussed here. If GDP losses are included in future versions, we will clarify that they represent constant price GDP in market exchange rate.
6502	6	30	30	30	36	This sentence should be eliminated. Because supporting evidence is not clear.	Rejected. An idealized policy scenario is definition, not an empirical finding.
9874	6	30	34			Remove the word "improbable" from this sentence - these scenarios are not forecasts - we can not assign probabilities to their occurrence, since they depend on future human decision making.	Taken into account. Changed to implausible.
8660	6	30	34			Remove the word "improbable" from this sentence - these scenarios are not forecasts - we can not assign probabilities to their occurrence, since they depend on future human decision making.	Taken into account. Changed to implausible.
9984	6	30	37	30	39	This part should be deleted completely because it is considered that IPCC should be policy irrelevant and has no recommended any integrated carbon markets until now.	Rejected. It is a misunderstanding that the discussion of idealized implementation scenarios is an endorsement of this approach or a policy recommendation. It is an important analytical tool to characterize costs.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16703	6	30	40		44	This should be deleted -- is based on the terrible misunderstanding that a lower carbon price is a demonstration of a lower cost policy set. In fact, a policy that requires additional deployment of a more costly resource regardless of the carbon price will act to lower the carbon price only because these more costly resources remove emissions - the market could have achieved those same emission reductions at a slightly higher carbon price, but lower overall total costs to the economy. Read the 1st paragraph on page 30.	Rejected. Some models show a significant reduction of mitigation costs due to, e.g., revenue recycling from a carbon tax (Waisman et al., 2012, Climatic Change 11(1)). The rest is a misunderstanding. We do not discuss the lowering of the carbon price, but that a carbon pricing policy combined with additional measures can reduce costs (It can also increase costs. See Section 6.3.5). The explanation has been clarified.
9872	6	30	5			In fact, here you state clearly my point above, which is that emissions prices are not representative of total costs because they represent marginal costs.	Noted
8658	6	30	5			In fact, here you state clearly my point above, which is that emissions prices are not representative of total costs because they represent marginal costs.	Noted
9417	6	30	33			Policy makers and researchers will be also interested in discussions on mitigation costs, however it is not easy to understand Figure6.13 & 6.14 & 6.15 and their explanations. It is recommended to restructure this section.	Taken into account - Figures revised
11257	6	31				I was wandering about the discretization of the x-axis in the cost figures. It is shown as equidistant, but is it equidistant in terms of forcing? In terms of CO2 budget? It is shown for the categories, but in what sense are they linear (as suggested on the x-axis)?	Noted. No linear relation of forcing between climate categories should be implied by the use of climate categories on the x axis.
5863	6	31				It is not clear what categories you refer to. Categories are given in 6.3.2.1 and 6.3.2.2, not 6.2.2. There are also more than 4 categories given there, and the numbers given in brackets with the categories in this figure do not match any numbers given with the categories in 6.3.2.1. and 6.3.2.2. Please clarify.	Taken into account. Use of categories will be harmonized. Categories 5&6 basically summarize baseline scenarios w/o climate policy, so discussion of mitigation costs will be restricted to Categories 0-4.
12019	6	31				The chozen discount rate of 5% looks a bit too high considering the current economic situation and future prospect. The costs can significantly by the value of discount rates. Higher discounts rates underestimate future costs. Therefore, sensitivity analysis of costs over discount rates should be included.	Taken into account. Information on the sensitivity of costs to choice of discount rate is now provided.
10396	6	31		31		The categories mentioned in this figure does not coincide with table 6.2. And the definition of the categories is given in section 6.3.2 not section 6.2.2.	Taken into account. Use of categories will be harmonized.
11423	6	31	1	31	12	Further explanations and clarifications should be provided to ensure that the use of an idealized implementation framework for mitigation will not be taken by readers as an implicit or explicit endorsement of such framework as a policy recommendation on the part of the IPCC AR5.	Taken into account. Explanation added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13146	6	31	11	31	12	it would also be important to acknowledge that cost estimates are already such a specific indicator that the large uncertainties visible in the model outcomes are probably dwarfed by the uncertainties that are unavoidable, when complex systems (e.g. global economy, innovation and technology development etc) are projected far into the future (i.e. level of knowledge concerning, not only probabilities, but also possibilities. Modelling of disruptive surprises, which are almost certainly going to take place, but the nature and timing of which is unknown). In other words, it is very unlikely that the range in model outcomes is a good representation of the actual uncertainties. At best it may show how relatively small differences in assumptions can already lead to large variations (i.e. presumably most modelling teams have tried to pick parameter values and trajectories that represent a "best guess" estimates of some kind).	Noted. Will be considered as we adjust the text in the next draft.
16704	6	31	12			you mention the uncertainty of the benefits of climate mitigation -- where do we estimate the value of the benefits? This would be a good time to point the reader to that work.	Taken into account. Reference to WGII is provided.
5864	6	31	16	32	15	It is not clear why you show two panels here. Why do you show two time frames? It has to be expected that longer periods of time - when included in an analysis - result in higher absolute or discounted costs. Please explain or delete one frame. In addition, figure 6.14 can be deleted as the content is included in 6.13 -the explanation in the text is sufficient.	Rejected. Information on a medium term and a long term time frame is both relevant. Figure 6.14 is not redundant, because it describes consistent cost increases between scenarios from single studies. This information cannot be gleaned from Figure 6.13.
8662	6	31	16			I hope these two figures do not mix the two or more kinds of cost outputs from different kinds of models together. If they do, please create separate figures for comparable types of cost calculations.	Taken into account. Different cost measures are provided explicitly now.
9876	6	31	29			Again, mitigation costs will not necessarily increase significantly with the stringency of climate stabilization. This result will depend on many assumptions, especially the costs assumed for fossil fuels as a function of the demand for such fuels. With a steep enough fossil fuel cost of supply curve, mitigation costs could decrease with greater climate change mitigation. Please make it clear that the runs done by modeling teams may have shown this result because of the cost assumptions and trajectories input to the models. Or, the statement could be modified to be "with any given set of input assumptions, the net costs of mitigation will increase (or become less negative) with the stringency of climate stabilization". Then the statement becomes a mathematical truism. Moreover life style changes and the mentionned Kaya decomposition components have to be considered, too.	Noted. It is already stated that the result of an increase in costs with stringency holds "in general". The figures and the statements are revised.
8663	6	31	29			Again, mitigation costs will not necessarily increase significantly with the stringency of climate stabilization. This result will depend on many assumptions, especially the costs assumed for fossil fuels as a function of the demand for such fuels. With a steep enough fossil fuel cost of supply curve, mitigation costs could decrease with greater climate change mitigation. Please make it clear that the runs done by modeling teams may have shown this result because of the cost assumptions and trajectories input to the models. Or, the statement could be modified to be "with any given set of input assumptions, the net costs of mitigation will increase (or become less negative) with the stringency of climate stabilization". Then the statement becomes a mathematical truism.	Noted. It is already stated that the result of an increase in costs with stringency holds "in general". The figures and the statements are revised.
8664	6	31	33			Why should any model runs be excluded from the graphical presentations as long as they use comparable costs?	Taken into account. The figures now include all models and the ranges are provided in the text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9875	6	31	9			I believe that this is the first place that assumptions about technology cost and performance is mentioned in this chapter. (Please check.) As noted above, this topic should be discussed up front, right after model structure. Please provide a table here or there with some key technology costs as assumed by different modeling groups so the reader can get a feel for how different there cost assumptions are. My recollection is that they can be quite different. The fact that different input assumptions, especially for the cost of technologies as well as for the cost of fossil fuels, will have a significant impact on the total net incremental costs or benefits of mitigation should be strongly highlighted so that the reader understands it is not just the differences in model structures which cause differences in total costs and benefits.	Taken into account. The text is re-arranged and the implications of technology portfolios is discussed in the consecutive section.
8661	6	31	9			I believe that this is the first place that assumptions about technology cost and performance is mentioned in this chapter. (Please check.) As noted above, this topic should be discussed up front, right after model structure. Please provide a table here or there with some key technology costs as assumed by different modeling groups so the reader can get a feel for how different there cost assumptions are. My recollection is that they can be quite different. The fact that different input assumptions, especially for the cost of technologies as well as for the cost of fossil fuels, will have a significant impact on the total net incremental costs or benefits of mitigation should be strongly highlighted so that the reader understands it is not just the differences in model structures which cause differences in total costs and benefits.	Taken into account. The text is re-arranged and the implications of technology portfolios is discussed in the consecutive section.
12103	6	31	28	31	28	This statement "A further observation is that the costs of mitigation are highly dependent on the level of stabilization; that is, mitigation cost estimates increase significantly with stringency of climate stabilization" - is not true as shown by Figure 1 in Schneider, S. and Azar, C. (2002) 'Are the costs of stabilising the atmosphere prohibitive?', Ecological Economics, vol 42, issues 1–2, pp73–80. http://stephenschneider.stanford.edu/Publications/PDF_Papers/EconomicCostsOfStabilizingClimate.pdf They showed that there was very little actual difference in "cost" irrespective of the stabilisation target. To understand why they explain " Top–down (economic) models typically suggest that the cost of a 50% reduction of global CO2 emissions from baseline by 2050 would cost some 1–4% of global GDP, and a 75–90% reduction by 2100 would cost some 3–6%. But since these studies also assume that global income grows by 2–3% per year, this abatement cost would be overtaken after a few years of income growth. Thus, the cost of 'climate insurance' amounts to 'only' a couple of years delay in achieving very impressive growth in per capita income levels. To be ten times richer (than in 2000) in 2100 AD versus 2102 AD would hardly be noticed and would likely be politically acceptable as an insurance." . Rather, as shown in IPCC AR5 Chapter 3, page 48, the Costs of Mitigation depend completely on the assumptions made in the modelling of the costs of climate change mitigation. For instance, if you accept that there have been historically alot of barriers to implementing energy efficiency opportunities.....as outlined in other parts of this assessment, then addressing those and requiring higher 2020 targets can actually lead to greater investment in energy efficiency by 2020 rather than less. This can have significantly positive economic effects as it leads to such large demand reductions that new power plants do not need to be built. For instance the 2011 Potsdam Institute for Climate Impact Research (PIK) study shows that it is more economically efficient, not less to for the European Union to aim for a 30 percent of greenhouse gas reduction by 2020 instead of the current 20% reduction target. A new study, led by the Potsdam Institute for Climate Impact Research (PIK), shows that a shift from 20 to 30 percent of greenhouse gas reduction by 2020 would boost the European economy and create 6 million jobs. The full study is available from http://www.european-climate-forum.net/fileadmin/ecf-documents/Press/A_New_Growth_Path_for_Europe__Synthesis_Report.pdf	Noted. The models that submitted their results to IPCC database show the described increase.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12105	6	31	4	31	6	"This difference in costs may be traced back to a range of assumptions embedded in the structures of the individual models,." Does not have a reference - suggest also refencing this point as it is a crucial one - Weyant, J. (2000) An Introduction to the Economics of Climate Change Policy, Stanford University, Repetto, R. and Austin, D. (1997) The Costs of Climate Protection: A Guide for the Perplexed, World Resources Institute, Washington, DC.	Noted. A reader is alerted to a discussion in the consecutive text.
11258	6	32				I was wandering about the discretization of the x-axis in the cost figures. It is shown as equidistant, but is it equidistant in terms of forcing? In terms of CO2 budget? It is shown for the categories, but in what sense are they linear (as suggested on the x-axis)?	Noted. No linear relation of forcing between climate categories should be implied by the use of climate categories on the x axis.
12020	6	32				same as above.	Noted. No linear relation of forcing between climate categories should be implied by the use of climate categories on the x axis.
13149	6	32	16	32	22	As mentioned previously, I would expect the the energy (and other) systems of 2100 to look quite different from the ones today, no matter whether a mitigation target is assumed or not. Also mentioned previously: It's hardly surprising that there's less variation when the absolute distance from the reference point (i.e. the baseline) is smaller, especially if one expects, as was suggested, the costs (as a function of the target) to increase faster than linearly.A lower variation for more stringent targets would imply significantly differently shaped implicit MAC curves for the models. Therefore, an increasing variation with more mitigation could be interpreted as "magnitude uncertainty" dominating (which seems to be the case, also based on figure 6.15) and larger variation with low targets as "shape uncertainty" dominating the variation across the models (with the previously mentioned strong caveats concerning the how complete this description of uncertainty can be expected to be).	Noted and will be considered as we adjust the text in the next draft.
16705	6	32	21		22	Suggest replacing last sentence with this: "Stringent scenarios require a more rapid replacement of existing plant and equipment as well as the deployment of some technologies before their costs have declined through a more gradual early stage deployment." [explanation: requiring 100 MW of solar in 2002 would cost a great deal more than requiring 100 MW of solar in 2012. Trying to go very fast pushes deployment at a time when much more could be achieve at a much lower costs if we had simply waited a little while longer.]	Noted and will be considered as we adjust the text in the next draft.
8666	6	32	31			The possibility that total net abatement costs could be linear with respect to cumulative levels of abatement seems somewhat strange for any given model. It would seem to only occur if there was one basic technology at a constant unit cost that could be relied on for abatement throughout this range. If more than one technology was called on for abatement over the range cited, then one would expect an increase in the slope of the cost curve when the penetration of the first and cheaper technology was saturated, reflecting higher marginal costs for the second technology. Presumably, such a change in slope would happen many times for any given scenario. Thus, a general linear trend as displayed in figure 6.15 must be an artifact of having outputs for many different models with many different sets of assumptions, and probably has no significance.	Taken into account. The rates of increase in costs are different among different classes of the models.
16706	6	32	33			insert after 2010-2100 "delaying the achievement of the 2100 consumptions levels in the no policy case by X months."	Comment is noted and will be considered as we adjust the text in he next draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8665	6	32	7			I find figure 6.14 confusing if the main point is to show cost variation with increased stringency across modeling groups. Again, the cause of these variations are many fold, different model structures, and different sets of input assumptions being two key causes. It is not clear to me that it is necessarily the case that the variability of costs will always increase with stringency for any set of input assumptions.	Taken into account - Figure will be beefed up with additional information or removed.
6537	6	32	20		21	Replace "bring greenhouse gas emissions toward zero" with e.g. "reduce greenhouse gas emissions significantly in accordance with AR4 WG1 Report Figure 10.22, or give a reference paper.	Taken into account. The text is adjusted.
6274	6	32	7	32	15	Delete Figure 6.14 and the text under the graphic. Keep the text at the bottom of page 31 and top of 32 that explains the take home point from this graphic. The text is far easier to interpret and digest than this graphic and its unit of measure of "mitigation costs relative to CatIII cliamte policy." Here's the rare case where a couple of sentences are better than a graphic. This will save about half a page.	Taken into account - Figure will be beefed up with additional information or removed.
16707	6	33	16		26	This discussions is extremely important because of how is frequently interpreted -- it creates a barrier for negotiators. It presumes no efforts are possible to address the regional disparities through the allocation of CO2 endowments which can be used to facilitate trade. The trade in this endowment, which can be based on BAU emissions until a country achieves a level of per capita GDP, can create the financial flows and ability to buy technology needed by developing countries. See work by Bossetti and Frankel http://belfercenter.ksg.harvard.edu/files/bosetti-frankel-dp-46-final-1.pdf .	Noted and will be considered as we adjust the text in the next draft.
11424	6	33	21	33	26	The assertion that the majority of emissions reductions in the 21st century will be borne by because their emissions are projected to be larger than those of developed countries needs to be explained more clearly in terms of what the assumptions are underlying such assertion. Absent a clear explanation of the assumptions for this assertion, such a bare assertion could be used in a non-scientific and political way in order to push specific policy agendas or approaches in the context of international policymaking discussions and negotiations on climate change that could effectively absolve developed countries of any further mitigation commitments and increase the pressure on developing countries to undertake increased mitigation actions. Furthermore, declarative formulations such as "the majority of emissions redutions over the coming century will be borned by the currently developing countries" should be avoided in order to prevent the use of such declarative statements in a political setting, particularly since such declarative statements might be construed by eventual readers as an implicit policy recommendation on the part of IPCC AR5 with respect to how future emissions reductions responsibilities should be allocated. This would bring the IPCC AR5 conclusions into the realm of policymaking rather than science.	Noted. The statement is supported by projections from a vast majority of the models.
16708	6	33	27		35	Very important -- highlight or move forward in document. Include ref to work by Bossetti and Frankel.	Noted and will be considered as we adjust the text in the next draft.
13150	6	33	31	33	34	Isn't this a bit too self evident to count as a finding?	The reference is provided to support the point better.
17233	6	33	34			Lüken et al is not found in the reference list. The reference makes sense here, though.	Taken into account.
14400	6	33	6			Need a more complete explanation of why the red dot studies are so different. Why are they included at all (rather than just mentioned in a footnote) if they are so unrepresentative? If this is the McKibben et al model only, I'd be reluctant to feature it as much as is done here. (See the doubts in Cline, 2011, p. 50-51). I think it is mixing short-term monetary issues in with long-term climate and technology issues.	Noted. The text is already edited and will be considered for further adjustments in the next draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4200	6	33	8			Very interesting figure. However, the non-linearity between cummulative carbon emission and cost in NPV is not so clear. Isn't log-scale graph better?	Noted. Log-scale will compromise comprehensibility of the figure for the lay person, and is not necessary, since only one order of magnitude is covered.
9571	6	34				Please, don't cap new nuclear capacity as explicit factors in the model in the case of no new nuclear case. When it is difficult to remove cap for it, please explain models' capacity prerequisite for no nuclear case in the text.	The comment needs clarification.
16709	6	34				Line 7 of description -- you mention compensation mechanisms. You should help define or explain somewhere in the report what these compensation mechanisms could be -- in particular the assignment of an allowance or CO2 endowment along a BAU pathway for developing countries. Very helpful if you could highlight this.	Taken into account. The text on burden sharing regimes and financial transfers is now in the new Section 6.3.6.6.
12021	6	34				The amounts of GHG reductions by marginal costs should be given before presenting this kind of analysis.	Figure 6.16 shows total costs rather than marginal costs. There is no Table 6.16.
9280	6	34	14	34	15	This table is duplicative of Table 6.ES.2 (Chapter 6, page 7 line 6).	Noted. The text will be adjusted in the next draft.
14401	6	34	2			See Cline (2011) estimates, p. 84. If cuts are to equal per capita emissions, the percent cuts are greater for industrial countries. Costs are broadly similar between the two groups.	Noted and will be considered as we adjust the text in the next draft.
11749	6	34				The condision of new nuclear should be described in this section or added the remark on the table 6.4. Refer to No.40.	Noted and will be considered as we adjust the text in the next draft.
3146	6	35	1			SECTION 6.3.5 on policy design overlaps with other chapters that cover similar material. TSU NEEDS TO ADVISE ON WHERE POLICY DESIGN ISSUES WILL BE HANDLED CENTRALLY.	Noted. The text is adjusted and most of the description is now in Chapter 3.
6100	6	35	15	35	17	In view of the importance of technology in mitigation, effect of a policy to promote technology innovation/diffusion shoule be included as one of the criteria. However, as I know that this matter should be discussed in Chapter 3, I have already made a comment for the Chapter 3 text.	Taken into account. Policy design issues are moved to Chapter 3.
6101	6	35	17	35	18	The explanation of economic efficiency here seems to be the same as cost-effectiveness. Actually, economic efficiency has something to do with the concept of optimization where marginal cost equalizes to marginal benefit.	The concepts of economic effciency and cost effectiveness are different. Concept definitions are now moved to Chapter 3.
8667	6	35	25			This sentence does not require a reference since it is a simple logical truth.	Noted.
7682	6	35	34	35	34	The text "implications are discussed in Section 6.2.6" should probably refer to Section 6.3.6?	Taken into account. The sections are re-arranged now.
13278	6	35	35	35	37	This sentence is unclear - the 20% reduction would lead to 0.5-2.0% of what? Presumably change in welfare; if so, please spell this out in the text	Taken into account. The reference is to welfare cost.
13151	6	35	36	35	37	Does "the lowest possible cost" here refer to using a single carbon price? If yes, make sure the terminology agrees with what is on page 30, lines 40-44.	Yes. The statement is qualified by adding "in the absence of other distortions".
9877	6	35	37			0.5 - 2.0% of what? GDP? Again, this result depends on input assumptions. It could be negative with sufficiently high fossil fuel prices in the base case.	The reference here is to welfare (not GDP). The intent is to show the impacts of expanding emission trading, rather than providing the exact magnitude of policy costs.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8668	6	35	37			0.5 - 2.0% of what? GDP? Again, this result depends on input assumptions. It could be negative with sufficiently high fossil fuel prices in the base case.	The reference here is to welfare (not GDP). The intent is to show the impacts of expanding emission trading, rather than providing the exact magnitude of policy costs.
16711	6	35	40			Insert at end of line: "These figures demonstrate the significant economic penalties that can be created as market structures deviate from the ideal or are over-engineered to satisfy other policy objectives."	Noted. The text is adjusted and some figures are deleted.
16710	6	35	5			suggest insert at end of sentence, after the word "market" the following: "participants as they operate to maximize utility."	Noted and will be considered as we adjust the text in the next draft.
7686	6	35	6	35	12	The introduction mentions that the subsection considers economic rents, but the word is not used in the rest of the subsection. Please ensure that the introduction is in line with the rest of the 6.3.5.x subsections. The text "[…] something that economists call a scarcity rent [...]" is too didactic for a review text. Also, the use of "we" (lines 10-12) feels inappropriate for IPCC assessment report.	Taken into account. The text is revised.
7685	6	35	1			The subsection (pp. 35-37) is very fragmented, and doesn't give a comprehensive view on different policy structures and their possible implications. The underlying problem is that the chapter is based on scenario modelling studies, and a IAM's have limited capacity to actually model different policy structures. The topics covered in the subsection are too narrow to warrant such a broad title. From the title I would e.g. expect a discussions on carbon tax vs. quantity targets, multilateral agreements and national policies, flexibility mechanisms etc. Policy agreements are covered more comprehensively in Chapter 13. Do the section's figures support the text and conclude the main findings? The section has some overlap with section 6.3.6, and also probably with various sections in Chapter 13. Perhaps this section could be merged with 6.3.6, as Chapter 6 requires shortening.	Taken into account. Policy design issues are moved to Chapter 3 and the current section is re-arranged.
6275	6	35	35	36	6	Consider deleting Figure 6.17 and the text under the graphic but keep the paragraph on Page 35 that starts with the bold face "Figure 6.17." That is indeeed a good example of the point being discussed in the third paragraph on page 35. The text in the short paragraph at the bottom of page 35 nicely states what the experiment was and the key results and insights. Figure 6.17 doesn't add much and in fact raises more issues that dont need to be dealt with here (e.g., what is DART, what is No. N+1, ...)	Noted and will be considered as we adjust the text in the next draft.
12309	6	35	1			In this section it is also important to look at how to handle risk sharing in a situation where there is a need for a shift in technology. There is more risk and higher cost involed for the early movers. Hence, this might create a need for other policies, especially in the transformation phase. This perspective is not necessarily taken care of by economic instruments, as the section seems to focus mainly on cap-and-trade and carbon text. It is important to convey the findings which is relevant for policy makers when developing the policy instruments.	Noted and will be considered as we adjust the text in the next draft.
13152	6	36	11	36	12	Figure is missing (6.17 shows something completely different, as does 6.18)	Taken into account. The figures are re-arranged.
9878	6	36	12			The 90% increase cited must depend on very specific input assumptions. If so, I would suggest carefully qualifying the statement as it appears to undermine the idea of establishing RPSs at any regional level.	Noted and will be considered as we adjust the text in the next draft.
8669	6	36	12			The 90% increase cited must depend on very specific input assumptions. If so, I would suggest carefully qualifying the statement as it appears to undermine the idea of establishing RPSs at any regional level.	Noted and will be considered as we adjust the text in the next draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9879	6	36	16			Again, the sentence that starts "it underscores the potentially large costs.. " seems quite politically explosive to me, and I am quite skeptical that it is generally true. If you want to make such powerful and sweeping statements I think more research needs to be described as to under what conditions a statement like this is true or not. Even if you reveal the underlying studies, I would avoid such a term, as it is too vague. This conclusion could be cited to oppose any type of sector specific policy and decisions on the company level. Yet, sector specific policies are usually much easier to implement and much more successful than "broad policies" like a carbon tax, if that is what is meant.	Noted and will be considered as we adjust the text in the next draft.
13153	6	36	16	36	17	I'm not entirely sure I understand this; to me all the CAT scenarios appear to have similar costs (i.e. circle areas), no matter what is assumed for FES. Or does this conclusion refer to the size of the "non-mitigation" circle vs "mitigation" circles, in order to point out that just reducing gasline use with FES, without achieving significant emission reductions, would cost ~ quarter of what it would cost to achieve at least as significant gasoline use reductions AND more meaningful mitigation outcomes? Elaborate a bit, so that it's clear what one should conclude from the figure.	Taken into account. The figure is deleted.
8670	6	36	16			Again, the sentence that starts "it underscores the potentially large costs.. " seems quite politically explosive to me, and I am quite skeptical that it is generally true. If you want to make such powerful and sweeping statements I think more research needs to be described as to under what conditions a statement like this is true or not. This conclusion could be cited to oppose any type of sector specific policy. Yet, sector specific policies are usually much easier to implement and much more successful than "broad policies" like a carbon tax, if that is what is meant.	Noted and will be considered as we adjust the text in the next draft.
5865	6	36	23	37	7	Including emissions from land-use is a delicate topic because most studies I am aware of do not reflect e.g. a "bug" in the reporting guidelines, but assume their assessment scheme to be correct. Losses of C in biomass are attributed to harvest activities and would have to be paid for by e.g. farmers and foresters. They in turn would have to raise prices to be remunerated. Thus prices for agricultural crops and timber will raise and, especially with timber, uses which generate more C emission reduction by replacement / substitution may be offset because of reduced harvests.	Noted. The challenges are mentioned and discussed in the provided references.
8671	6	36	23			Addressing land-use related abatement will only reduce overall abatement costs if the marginal costs of land-use abatement measures are lower than the marginal cost of the last abatement technology that would have otherwise been relied on. This is another example of how conclusions need to be properly qualified.	Noted. There is a range of emission abatement options from land-use change. With a certain carbon price, some actions will be justified, as reflected in the provided references.
7687	6	36	7	36	9	The text statest rather blatantly that "The most economically-efficient climate policy remains cap-and-trade policy or carbon tax", with a single reference at the end of the statement. While I'm certainly not against this statement in itself, I think the tone is rather arrogant and unbalanced. I order to prove the point, it is better to cite results from studies that have compared the efficiency of market-based to other regulatory measures.	Taken into account. The statement is re-arranged and additional references to EMF studies are considered.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11750	6	36	7	36	9	Disagree. Some says volantay acions are effective and C&T, tax are not necessarily effective.The sentence [The most economically-efficient ....] should be deleted.Refer to Okazaki et al.,Wakabayashi et al., Montgomery et al. 1.Okazaki et al.:[Accelerating the transfer and dissusion of of energy saving technologies steel sector experience] send attachment by another e-mail. 2.Wakabayashi et al:[Case Studies and Its Effectiveness of Environmental Taxation], http://criepi.denken.or.jp/en/serc/research_re/download/09005dp.pdf 3.Wakabayashi et al.:[A Review on Effectiveness of Emissions Trading Schemes: Empirical Evidences of Their Implementation], send attachment by another e-mail. 4.Montgomery et al.:[Price, Quantity and Technology Strategies for Climate Change Policy], http://crai.ca/uploadedFiles/RELATING_MATERIALS/Publications/Consultant_publications/files/pub_4141.pdf	Noted. Distorted implementation of carbon tax and cap-and-trade may increase costs. The qualified is added.
16712	6	36	7		17	These points are very important and should be moved directly to the executive summary.	Noted and will be considered as we adjust the text in the next draft.
10647	6	36	7	36	9	There is a lot of arguments that volantary actions are more effective. Please see Okazaki et all, Wakabayashi et a and Montgomery et al. 1.Okazaki et al.:[Accelerating the transfer and dissusion of of energy saving technologies steel sector experience], send attachment by another e-mail. 2.Wakabayashi et al:[Case Studies and Its Effectiveness of Environmental Taxation], http://criepi.denken.or.jp/en/serc/research_re/download/09005dp.pdf 3.Wakabayashi et al.:[A Review on Effectiveness of Emissions Trading Schemes: Empirical Evidences of Their Implementation], send attachment by another e-mail. 4.Montgomery et al.:[Price, Quantity and Technology Strategies for Climate Change Policy], http://crai.ca/uploadedFiles/RELATING_MATERIALS/Publications/Consultant_publications/files/pub_4141.pdf	Noted. The literature on effectiveness of voluntary actions for stringent emission reductions is non-existent.
9985	6	36	7	36	9	This part should be deleted completely because there are successful examples of "voluntary target scheme" in the world. Each industry in Japan has voluntary target and the voluntary target scheme has played a big role, as described in (Yamaguchi, 2012, page35 and 154), (Manuel, 2010, page 6 and 13), and (Yamaguchi, 2010, abstract). In addition, there is also a successful example of "voluntary target scheme" in Netherlands, as shown in (Martijin, 2002, page162). These literatures are listed in the No63 line of this table. On the other hand, market-based mechanism such as emission trading has several problems. Volatility of emission permit prices affects volatility of product prices as evidenced by fluctuating price developments in the EU-ETS. Therefore, the market-based policy tools of cap-and-trade cannot provide credible incentives for the technological change, as described in (Montgomery, 2005, abstract) and (Baldursson, 2009, page29). These literatures are listed in the No62 line of this table. In addition, CO2 leakage caused by the implementation of the ETS happened actually through international transfer of industry , as shown in (Rosendahl, 2011, abstract), (Aichele, 2012, page336), and (Peters, 2011, page1). These literatures are listed in the No50 line of this table.	Noted. The literature on effectiveness of voluntary actions for stringent emission reductions is non-existent.
9365	6	36	7	36	9	It should be deleted because there is a successful example of the voluntary action of the Japanese steel industry Also carbon tax does not always seem to be effective. Wakabayashi et al showed that the difficulty to keep tax rate in reality because of the price competitiveness in industrial sector.(Wakabayashi and Sugiyama)	Noted. The literature on effectiveness of voluntary actions for stringent emission reductions is non-existent.
6498	6	36	7	36	9	This sentence should be eliminated. It is hard to mention that the most economical-efficient climate policy is cap-and-trade policy or carbon tax. Because the supporting evidence is not clear and the problems are pointed out in 6.3.5.2	Noted. Distorted implementation of carbon tax and cap-and-trade may increase costs. The qualified is added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16713	6	37	20		21	Sentence beginning with "different allocation schemes …" is very important. These points should be moved forward and included in the executive summary.	Noted and will be considered as we adjust the text in the next draft.
16714	6	37	23		26	This lacks context -- how big will the changes be and what will be their impact?	Noted. We consider deleting this discussion for limited space reasons. If kept, we consider adjusting the text in the next draft
8046	6	37	27	37	38	a reference to chapter 6.3.6.2 is helpful	Noted and will be considered as we adjust the text in the next draft.
13154	6	37	31	37	31	Same effects as what?	Same fuel and emission reduction. The text is edited now.
8672	6	37	31			"same effects" as what???	Same fuel and emission reduction. The text is edited now.
9880	6	37	33			Again, I think that the estimate of 2-10 times the cost (of what scenario? A cap and trade system?) should be very carefully justified and qualified, because otherwise it can be mis- used by opponents of almost any climate policy that is not a cap-and-trade system or carbon tax. Cost multiples at this high a magnitude are suspect to me because they are produced by economic modules that have very aggregate treatment of each economic sector. It is not clear, therefore, what kinds of sectoral-specific climate mitigation policies they could even model reasonably accurately.	The text is edited.
8673	6	37	33			Again, I think that the estimate of 2-10 times the cost (of what scenario? A cap and trade system?) should be very carefully justified and qualified, because otherwise it can be mis- used by opponents of almost any climate policy that is not a cap-and-trade system or carbon tax. Cost multiples at this high a magnitude are suspect to me because they are produced by economic modules that have very aggregate treatment of each economic sector. It is not clear, therefore, what kinds of sectoral-specific climate mitigation policies they could even model reasonably accurately.	The text is edited.
11425	6	37	43	37	45	The assertion that developing countries "are new responsible for the majority of greenhouse gas emissions moving forward" is similar to assertions made elsewhere in the chapter about the potential future role of developing countries as the main contributors to such emissions and hence should have the main future responsibility for reducing emissions. As with such other assertions, this assertion should also be clearly explained in terms of what the assumptions were, so as to avoid the IPCC AR5 being read or construed in this instance as providing a policy recommendation with respect to the allocation of future emission reduction commitments.	Will take this into account and rephrase in a way that it is clear that no policy reccomandation is made
6103	6	37	45	37	45	The word "especially" should be added before "at the international level". This situation also happen domestically	noted
6102	6	37	8	37	26	Very important message that distributional impact affects feasibility of a policy instrument is missing.	Noted and will be considered as we adjust the text in the next draft.
7688	6	37	8			Distributional impacts are likely to be a huge global issue. Starting with a study on US households seems very biased. Perhaps the text could start by stating that impacts vary by policy, region and individual, with references to each. With cap-and-trade and taxes with transfers, the economic impacts can be adjusted. There is a large amount of literature on burden sharing (or effort sharing), but this is covered with merely a short note on allocation schemes.	The text of burden sharing and financial transfers is now in the new section 6.3.6.6.
7689	6	37	27			This important topic deserves much more comprehensive treatment. The whole subsection has been written from a viewpoint of an economist, althought it is more in the field of a policical scientist (not political economist). Anecdotal evidence from the US seems inappropriate. Is there any literature on e.g. the UNFCCC process and what kind of mandates the negotiators there have?	The text is re-arranged. The sub-section is removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7690	6	37	39			The subsection (pp. 37-42) deals mostly with scenario studies on international participation to the global climate policy, but this is a very narrow scope compared to the subsection's broad title. Policy agreements are covered more comprehensively in Chapter 13. The subsection is also quite heavy on figures relative to the amount of text. In addition, some of the figures are not referenced in the text. Are the figures representative of the literature on that covered topics, and do they conclude the main findings from scenario studies on international strategies for mitigation? Perhaps section 6.3.6 could be merged with section 6.3.5 as there is some overlap between them.	This chapter deals with modeling and the issue of international agreements is covered inso far models have addressed it in the literature. Chapter 13 does not provide quantitative estimates of the role of international participation, so there in our view value of keeping this section alive.
16715	6	38	10		30	These points are extremely important -- need greater emphasis and should be included in exec summary -- they are buried in body of report and risk being overlooked.	The chapter will be reorganized so as to give this section more visibility.
9499	6	38	12	38	21	good issue - All of the countries should take part in climate stabilization activities	thanks
5924	6	38	12		30	This could be added as well. It supports the overall message given in this chapter. Ekholm T., Soimakallio S, Moltmann S., Höhne N, Syri S., 2010. Effort sharing in ambitious, global climate change mitigation scenarios. Energy Policy 38(4), 1797-1810.	noted
7691	6	38	16		20	There is no need to elevate EMF22 above other participation studies in an IPCC assessment report ("one of the most comprehensive assessment of this issue"). Please reformulate the text so that it states the main findings of EMF22, and include also results from other participation studies that are referenced.	More studies will be added in the SOD.
11426	6	38	20	38	24	The bases and assumptions for why "many models were not able to produce scenarios with delayed participation of large developing countries for the more stringent long-term goals" should be further explained and clarified. Furthermore, a more balanced framing of the argument should be used. For example, instead of stressing only that "half of models found it impossible to meet the 550 Co2-e target with delayed participation", an additional phrase could be added to say to present the other side of the picture such that "on the other hand, half of the models deem it possible to meet such target with delayed participation from developing countries" (that is, if the literature would allow such an assertion to be made).	noted
7692	6	38	26		27	Please clarify/elaborate "model cannot be solved" and the "high initial price".	We will add more information on the notion of infeasibility
8674	6	38	29			Again, the second two reasons for apparent infeasibility may also reflect the overly constrained amount of energy efficiency improvements allowed per year in these models, as well as other input constraints that are not absolutely firm.	We will add more information on the notion of infeasibility
6692	6	38	10			Good text.All nations should make efforts to control too rapid climate change. So, It is effective to construct a framework under which all nations, including developing countories, have a responsibility to reduce carbon emission.	thanks
6276	6	38	10	38	30	The material discussed in section 6.3.6.2 is very important and is one aspect of what is new that is in the literature that wasn't probably addressed in AR4 in detail. It is concerning to this peer reviewer that this information is 38 pages in this chapter. Serious consideration needs to be given to condensing the many pages of important caveats about what IAM models can and can not do so that insights like this are not lost. The sentence about not being able to get to 550 if large regions stay out is a potential candidate for the executive summary or an FAQ at the end of the chapter.	The chapter will be reorganized so as to give this section more visibility.
15220	6	39				Figure 6.19 needs to be revised to make it clear for understanding.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4201	6	39				hard to read!	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
6538	6	39				Complete Figure 6.19 and give a reference paper.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
5866	6	39				Rework, not legible.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
6104	6	39				Can not read.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
10793	6	39	1			Figure is garbled and confusing. Please redesign	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
14455	6	39	1			This graph is illegible.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
3148	6	39	1			Figure 6.19 is illegible but seems to be important. I would suggest that the discussion of this figure include cross references to other chapters where international cooperation is addressed in detail such as chapters 2 and 13.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
14402	6	39	10			See Cline (p. 70) on advantages to China of earlier cutbacks than Copenhagen pledge, if cuts are to reach 1.4 tCO2 per capita by 2050	noted
9572	6	39	12	39	13	Please, replace advantageous terms of trade with avoidance of lock-in problem as advantageous terms of trade is unclear.	noted
16716	6	39	19			Suggest this point of discussion w/in the doc: Late participation also implies that developing countries miss the opportunity to negotiate advantageous emission pathways that could be monetized via trading as part of an international cap and trade program. This agreed endowment could be used as a source of capital which could then be used to buy needed mitigation technology. Entering later may reduce the value of this opportunity.	noted
6277	6	39		39		Figure 6.19 is not legible.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
15221	6	40				Figure needs to be revised to make it clear for understanding.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7683	6	40	1			The figure lacks y-axis label. The NPV/maximum loss should also be noted in the x-axis label to improve readability.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
9986	6	40	32	40	33	This part should be completely deleted. Market-based mechanism such as emission trading has several problems. Volatility of emission permit prices affects volatility of product prices as evidenced by fluctuating price developments in the EU-ETS. Therefore, the market-based policy tools of cap-and-trade cannot provide credible incentives for the technological change, as described in (Montgomery, 2005, abstract) and (Baldursson, 2009, page29). These literatures are listed in the No62 line of this table. In addition, CO2 leakage caused by the implementation of the ETS happened actually through transfer of industry from one country to others. Market mechanisms at least under Kyoto-like international scheme, where the condition of all countries' meaningful participation is not met, does not work well, as shown in (Rosendahl, 2011, abstract), (Aichele, 2012, page336), and (Peters, 2011, page1). These literatures are listed in the No50 line of this table.	Market based schemes are discussed in so far they can induce cooperation, not techhnical change.
16717	6	40	33			Suggest insert before sentence that begins with "The financial transfers that would result …" the following text: Emissions pathways for developing countries in a cap and trade system fore example need not immediately decline, but rather can follow a business as usual pathway until an agreed level of per capita GDP is achieved." Ref work by Bossetti & Frankel. Many do not understand this and highlighting it could be helpful.	noted
4202	6	40	9	40	29	The expression of these paragraphs seems slightly ambiguous for the readers. Are there some examples of such partial coalition and its inefficient outcome? What is important in this chapter is, to me, the need for the incentives for the participation.	More discussion will be added
11659	6	40				From the figure, GDP loss of fragmented participation seems to be smaller for other DCs than that of full participation in NPV terms, which means that the delay in participation is beneficial for other DCs. The rationale behind this result should be mentioned.	noted
10989	6	40	32	40	33	In what aspects could schemes like international emission trading be said it is quite successful in inducing cooperation? It should be supported by some facts. Reference: Jared C. Carbone, Carsten Helm, Thomas F. Rutherford, The Case for International Emission Trade in the Absence of Cooperative Climate Policy (2009).	The paper by Carbone et. al is already quoted.
9573	6	41				Please, describe expaination and different timing of participation from countries in the text or footnote as it is difficult to understand bar chart.	will improve the chart clarity
6539	6	41				Explain the deferent allocation schemes in Figure 6.22. and give a reference paper.	This figure will be replaced with an updated one
5867	6	41				Please explain regions (RAI = ?, RNAI = ?). Are negative losses gains? Or does the X-axis show changes, not losses?	This figure will be replaced with an updated one
8675	6	41	1			The results for average financial transfers in figure 6.21 look rather odd, in that for some models they are close to zero. Perhaps the text should explain the huge differences between different model results. How could the right answer be close to zero for any model?	Model variability is often an outcome of model ensamble analysis. This chart will be redrawan with new data, though some variability might remain.
9574	6	41	6			Please, add following information as the reason of previous sentense; average emissions in developed countries would grow more slowly or decline, while those in developing countries would increase more rapidly, which has an implication for individual countries burden.	will change the chart and the associated text
9881	6	42	0			In general, I would suggest that all the previous presentation of policy analyses go at the very end of the chapter. It would be logical to present the material in section 6.3.7 first as part of the basis for the policy results.	The chapter will be reorganized so as to give this section more clarity

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8676	6	42	0			In general, I would suggest that all the previous presentation of policy analyses go at the very end of the chapter. It would be logical to present the material in section 6.3.7 first as part of the basis for the policy results.	The chapter will be reorganized so as to give this section more clarity
16718	6	42	19	43	18	These are very unclear. Suggest rewrite.	Comment is noted and will be considered as the text is adjusted for the next draft.
13157	6	42	20	42	33	As the chapter is currently too long, I suggest this paragraph and the figure 6.24 that follows is cut. It's hardly surprising that emissions from fossil fuels strongly correlate with non-CCS related primary energy use of fossil fuels, nor that both go down with mitigation (or that there still can be variations across the scenarios). Sacrificing nearly a page to this does not seem necessary.	Figure 6.24 and the surrounding discussion has been replaced by a figure that relates climate targets to the use of fossil fuels.
4203	6	42	20	42	24	Resource availability and cost issues should be touched upon here.	Fossil fuel use under different climate targets is discussed more explicitly. Note that Section 7.4 in Chapter 7 deals with resource availability in more depth.
9882	6	42	25			This section briefly mentions "limits on the use of… fossil energy…", but does not really put the future of energy systems and technologies in its proper context which must include some discussion of the peak oil, peak gas, etc. debates. Even Chapter 1 of the WGIII report discusses this issue, and it should be picked up here because it could have a major impact on the prices assumed in each IAM for fossil fuels in the future, as a function of demand. My sense of the history of IAM modeling is that the modeling teams have not paid sufficient attention to this issue, and continue to model fossil fuel supplies and prices in much the same way as they did for the fourth IPCC assessment, even though the IEA and many other organizations have changed their views dramatically on the peak oil issue. Many claim that peak conventional oil production has already peaked in 2006, with important implications for the price of oil and other fossil fuels in the future. Thus, there needs to be a discussion in this section of how oil supplies are modeled by the various IAM teams, and back-up technologies for liquid fuels, etc.	Fossil fuel use under different climate targets is discussed more explicitly. Note that Section 7.4 in Chapter 7 deals with resource availability in more depth.
8677	6	42	25			This section briefly mentions "limits on the use of… fossil energy…", but does not really put the future of energy systems and technologies in its proper context which must include some discussion of the peak oil, peak gas, etc. debates. Even Chapter 1 of the WGIII report discusses this issue, and it should be picked up here because it could have a major impact on the prices assumed in each IAM for fossil fuels in the future, as a function of demand. My sense of the history of IAM modeling is that the modeling teams have not paid sufficient attention to this issue, and continue to model fossil fuel supplies and prices in much the same way as they did for the fourth IPCC assessment, even though the IEA and many other organizations have changed their views dramatically on the peak oil issue. Many claim that peak conventional oil production has already peaked in 2006, with important implications for the price of oil and other fossil fuels in the future. Thus, there needs to be a discussion in this section of how oil supplies are modeled by the various IAM teams, and back-up technologies for liquid fuels, etc.	Fossil fuel use under different climate targets is discussed more explicitly. Note that Section 7.4 in Chapter 7 deals with resource availability in more depth.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11427	6	42	3	42	13	This particular section seems to take the view that the current international climate policy regime is fragmented. This is not accurate considering that the UNFCCC actually provides for a coherent and comprehensive policy regime with respect to various aspects and actions that countries are supposed to do together in a cooperative manner, including on mitigation, adaptation, finance, and technology transfer. In the UNFCCC policy regime, roles and responsibilities are clearly demarcated and outlined. The problem lies not so much with the design and architecture of the UNFCCC policy regime itself but rather with how the various responsibilities and commitments arising from the policy regime have been fully or not fully implemented.	noted
10990	6	42	9	42	11	Why does "the long-term constraint enforces a degree of mitigation discipline" mean "to speed up mitigation efforts for the early entrants and delay them for the late entrants"? Why does such a situation happen? It is a little logically complicated, so it should be clearly explained.	will clarify
12106	6	42	18	43	26	As a general comment - Chapter 6 "Energy Sector Technology Transitions" is about technical system transformation - it ignores completely 2 key technical system transformation ideas - 1) That Whole of System Optimisation will achieve more cost effective end use energy efficiency than isolated technical stratgies - eg: As IPCC 2007 AR4 Building Chapter stated "Energy efficiency strategies focused on individual energy-using devices or design features are often limited to incremental improvements. Examining the building as an entire system can lead to entirely different design solutions. This can result in new buildings that use much less energy but are no more expensive than conventional buildings. The systems approach in turn requires an integrated design process in which the building performance is optimized through an iterative process that involves all members of the design team from the beginning." REF (Stasinopoulos, P., Smith, M., Hargroves, K. and Desha, C. (2008) Whole System Design: An Integrated Approach to Sustainable Engineering, Earthscan, London, UNESCO and WFEO at http://www.naturaledgeproject.net/Whole_System_Design.aspx) 2) There is a cross sector "energy" system synergies that will bring down the costs of transformation eg: namely the synergy between the transport sector (innovations in electric cars + batteries) and their potential to, through "Smart Grids", work with and enhance the transition the distributed renewable electricity supply. [Refs IEA (2011) Smart Grid Technology Roadmap. IEA at http://www.iea.org/papers/2011/smartgrids_roadmap.pdf + IEA (2011) Electric and Plug-In Hybrid Electric Vehicle Technological Roadmap. IEA http://www.iea.org/papers/2011/EV_PHEV_Roadmap.pdf] I have published on this and can send a summary through if interested.	As discussed in Section 6.2, the entire Chapter 6 is dealing with integrated analysis of mitigation and by construction deals with the intercation between energy demand and supply, but in addition also aims at capturing key interactions between the energy system and other human and natural systems.
10958	6	42	18	45	16	Confer: Torvanger, Lund, Rive, Carbon capture and storage deployment rates: needs and feasibility, Mitigation and Adaptation Strategies for Global Change, DOI: http://dx.doi.org/10.1007/s11027-012-9357-7	Reference will be included in SOD.
9418	6	43				Strong relations between fossil primary energy supply and energy-related CO2 emissions are obvious. Thus figure6.24 is not something new since AR4. However, amounts of energy supply and CO2 emissions in different categories are informative. Thus, it is recommended to revise this figure, maybe in time-series trend or to incorporate with Figure6.25 ?	Figure 6.24 and the surrounding discussion has been replaced by a figure that relates climate targets to the use of fossil fuels.
16719	6	43				What is significance of these graphs? Unclear.	Figure 6.24 and the surrounding discussion has been replaced by a figure that relates climate targets to the use of fossil fuels.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11751	6	43	10	43	18	Delete the sentence [and therefore with the long-term stabilization goal]. Figure 6.25 shows only the uncertainity for deployment of low-carbon energy in the future. The importance of low carbon energy itself never change. (refer to No.22).	There is a clear trend that low-carbon energy deployment increases with the stringency of the long-term climate target (and the decrese in emissions levels in specific years). However, given that the overall final energy use depends on the extent of energy service demand response and energy efficiency improvements there is a large range of possible low-carbon energy supply that is consistent with a specific climate target/emissions level.
10648	6	43	10	43	18	fossil fuel use and industrial processes (Figure 6.25) consists of a lot of uncertaities. It does not seem correlated but it is sure that the low carbon technologies still play an important role.	There is a clear trend that low-carbon energy deployment increases with the stringency of the long-term climate target (and the decrese in emissions levels in specific years). However, given that the overall final energy use depends on the extent of energy service demand response and energy efficiency improvements there is a large range of possible low-carbon energy supply that is consistent with a specific climate target/emissions level.
4204	6	43	19	43	26	The cost and the quality (convenience) of energy form issues should be important.	This point has been emphasized in SOD Section 6.3.4.2.
13159	6	43	7	43	26	Like the preceeding paragraph on fossil fuels, I find also these paragraphs (and the figure that follows) rather trivial and unnecessary. If must be possible to make in less than nearly two pages the rather simple point that mitigatio reduces the use of fossil fuels, increases the use of low carbon energy and, all else being equal, increases low carbon use even further if demand is high.	Comment is noted and will be considered as the text is adjusted for the next draft.
11263	6	43				I do not see the kind of information that Fig 6.24 provides, I think they are trival and therefore useless.	Figure 6.24 and the surrounding discussion has been replaced by a figure that relates climate targets to the use of fossil fuels.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10991	6	43	10	43	13	It is understandable that the use of low-carbon energy is far less well correlated with the CO2 emissions from fossil fuel use and industrial processes. However, the low-carbon energy could be rather far more well correlated with the long-term stabilization goal. Therefore, the expression of "and therfore with the long-term stabilization goal" should be deleted. Reference: S. Pacala and R. Socolow, Stabilization Wedges: Solving the Climate Problem for the Next 50 Years with Current Technologies (2004).	There is a clear trend that low-carbon energy deployment increases with the stringency of the long-term climate target (and the decrease in emissions levels in specific years). However, given that the overall final energy use depends on the extent of energy service demand response and energy efficiency improvements there is a large range of possible low-carbon energy supply that is consistent with a specific climate target/emissions level.
14684	6	430	46	431	1	It is not clear here what the intent of the comment on geoenegineering is in this case. What sort of reference will be made to geoengineering at this point?	Comment is unclear
16720	6	44	21			Suggest inserting: Modeling indicates that the cost of CCS technology is an important determinant in the price of CO2 in cap and trade policy scenarios. If CCS is expensive, the CO2 price under tight CO2 caps will be high. If CCS is relatively inexpensive, the CO2 price will be lower. While it may be possible to achieve transformation by relying solely on renewables and reductions in demand, modeling exercises suggest that the cost of such technology limitations is much higher than those scenarios that allow the use of all low carbon technologies, including CCS and nuclear energy. reference page 36 in this chapter.	The costs of CCS technologies is typically only one among many indicators that have an influence on the CO2 price. The overall portfolio of available options - on the supply and demand side as well as in other sectors (e.g., agriculture) - tends to be more important than economic assumptions of one specific technology.
6278	6	44		44		Of the two figures, 6.24 is clearly more informative and is more important to retain in this chapter than is the information in Figure 6.25. The text that describes Figure 6.26 adequately makes the point. If low carbon energy doesn't have emissions then the amount of low carbon energy that can be used could vary considerably across the models and the different climate sceanrios. That's a pretty straightforward point that doesn't need a half page graph and figure caption to communicate to the reader.	Figure 6.24 has been replaced by a figure that relates climate targets to the use of fossil fuels to link the discussion of climate targets with that on fossil fuel scarcity.
11264	6	44				the information of this figure is very limited, it could only be useful if you separate between CCS, RES and nuclear. I do not understand why this figure is in the Executive summary.	There is only limited scope for dealing with individual energy supply technologies within Chapter 6. A more detailed breakdown of low-carbon energy can be found in Seciton 7.12 of Chapter 7 on energy systems.
10992	6	44	11	44	16	Why could it be said "it is particularly tightly linked to the importance of fossil CCS in a specific pathway"? Even if we see the figure 6.26, we cannot find any concrete figure which supports the importance of fossil CCS. It should be explained more concretely.	Sentence has been removed.
14685	6	449	35	449	35	Is it fair to say that all proposed geoengineering strategies constitute adaptation? I'm not sure this is such a clear association	Any implication that geoengineering is equivalent to adaptation will be removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9575	6	45				Please, describe model's prerequite for nuclear deployment in the text as while the chart (b) moves towards top in renewables, IEA analysis shows generation by hi REN and hi NUC case in 2050.They seem to be different from results of chart (b) (ETP 2010, Table 3.1, IEA).	The scenarios assessed in Chapter 6 include high and low nuclear or renewable variants similar to those from IEA ETP 2010.
5868	6	45				Please rework the figure and shorten the text. There are no green or black letters legible, there are too many models (the different trajectories are not distinguishable) and using letters in the same colour as for shadings does not help to read a figure, too.	Figure has been reworked to highlight main points rather than showing individual model behavior.
8349	6	45	1		7	How about making two figures in a same axis? For example, coal, oil and gas are summed to fossils and Non fossils are divided to renewables and nuclear in (a) figure like (b) figure	Two primary energy ternary plots have been included in the SOD, one with the original split coal, hydrocarbons, non-fossil energy and another that splits low-carbon energy into CCS/nuclear, biomass, non-biomass renewables.
16721	6	45	17		25	Very important point -- make sure this is part of executive summary.	The point that electrification is a robust part of a mitigation strategy has been emphasized.
9180	6	45	17	45	25	It is very important, concrete policy relevant message to policy maker hence should be put in the exec summary.	The point that electrification is a robust part of a mitigation strategy has been emphasized.
9179	6	45	17	46	8	good argument. Also mention that the well-to-wheel efficiency is high for eletricity in many cases (heat pumps, EVs , etc)	The point that electrification is a robust part of a mitigation strategy has been emphasized.
11753	6	45	21	45	25	It is reasonable analysis.	The point that electrification is a robust part of a mitigation strategy has been emphasized.
10649	6	45	21	45	25	This is a good analysis.	The point that electrification is a robust part of a mitigation strategy has been emphasized.
9366	6	45	22	45	25	It raises an important point thus should be remained.	The point that electrification is a robust part of a mitigation strategy has been emphasized.
6500	6	45	22	45	25	This sentence should be left. Because it is described easily to understand that electrication of the end-use sectors is effective as a way of reducing GHG emissions. Even further description is needed.	The point that electrification is a robust part of a mitigation strategy has been emphasized.
11752	6	45	8	45	10	[public acceptance issues and other] should be amended to [some] because they aren't problem only for low-carbon technologies.	Sentece has been adjusted.
9576	6	45	8	45	10	Please, replace here with following as public acceptance is an issue for all types of generation; electricity generations, including low-carbon technologies, face public acceptance and other barriers that may limit or slow down deployment.	Sentece has been adjusted.
10993	6	45				In this figure, Electricity Generation Shares should be also classified in primary energy resources in order to analyze fairly.	The electricity figure has been removed and a more detailed discussion of electricity generation can be found in Section 7.12 of Chapter 7 on energy systems.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4773	6	45	8	45	16	It is true that some technologies, even low CO2 emission ones, are controversial, as all technologies have an impact on the environment. It is therefore important to develop those technologies under a sustainable way (i.e. in addition to pure technic issues, it is mandatory to identify and ahve support from stakeholders, deeply consultations, appropriate communications, etc.). I can provide examples on request, on the way to develop renewables or other technologies under a sustainable way.	Sentence has been adjusted. Section 6.6 includes a more detailed discussion of sustainable development in this context.
6279	6	45	8	45	8	To maintain consistency, replace "CO2 storage" with "CCS"	Language will be consolidated toward final draft.
12104	6	45	18	46	20	"Energy end use sectors along transformation pathways" misses a key point - the focus should be first on end use energy efficiency and demand management. Energy efficiency and demand management are critical to reducing electricity demand so that renewabel energy investment does enable overall GHG reductions in the electricity sector. If demand keeps rising, no matter how fast renewable energy is implemented, GHG will not be reduced fast enough to avoid dangerous climate change. As California has shown electricity demand can be flattened. The flattening of electricity demand changes the economics of electricity supply and makes the economics of distributed renewables much more favourable.... . This is because - Renewable energy systems are smaller than large centralised fossile fuel power plants and thus have both lower up front costs and shorter construction time reducing the cost of tying up capital unproductively or needing to rely on loans from banks. renewable energy systems can be built quickly enabling income to start flowing much more quickly than large centralised power plants which can take many years to build. (ref Lovins, A.B. et al (2002) Small is Profitable: the hidden economic benefits of making electrical resources the right size, Rocky Mountain Institute, Colorado, p 173. Available at www.smallisprofitable.org/) - Renewable Energy systems also also overcome the main financial risk of large centralised fossil fuel power stations namely that demand will not match the new level of supply. In cases when future demand fails to meet expectations, additional scheduled increments of renewable energy capacity can be foregone, avoiding the cost of overbuilt centralized capacity. (Ref Hoff, T.E. and Herig, C. (1997) 'Managing Risk Using Renewable Energy Technologies', in Awerbuch, S. and Preston, A. (eds)The Virtual Utility: Accounting, Technology and Competitive Aspects of the Emerging Industry, Kluwer Academic, Boston. Available at www.cleanpower.com./research/riskmanagement/mrur.pdf) □	In the literature both efficiency focused and supply-side focused approaches have been analyzed which is described here. A statement that prioritizes one over the other cannot be supported from the available literature.
6280	6	45	21	45	25	There are many papers going back many years that make this point. It is fine to cite the Sugiyama 2012 paper but there should be a number of other papers cited here as well. The value in citing more than one paper for a point like this is to clearly communicate to the reader that this is a well established point and is not something that can be dismissed as a fluke result that came from only one model.	Other papers have been added.
5869	6	46				Please rework the figure and shorten the text. There are no green or black letters legible, there are too many models (the different trajectories are not distinguishable) and using letters in the same colour as for shadings does not help to read a figure, too.	Figure has been reworked.
9883	6	46	11			When you say "economically efficient" do you mean that the net benefits are positive (negative net costs) or do you just mean that the net cost of the energy efficiency related demand reductions if less than the marginal costs of other mitigation supply-side technologies? Please clarify and explain. This might also be a good place to explain the basis for the levels of energy demand reductions allowed in most models.	This interpretation is correct. We will consider clarifying this statement further within space limitations.
8679	6	46	11			When you say "economically efficient" do you mean that the net benefits are positive (negative net costs) or do you just mean that the net cost of the energy efficiency related demand reductions if less than the marginal costs of other mitigation supply-side technologies? Please clarify and explain. This might also be a good place to explain the basis for the levels of energy demand reductions allowed in most models.	This interpretation is correct. We will consider clarifying this statement further within space limitations.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8678	6	46	17			The fact that the carbon intensity declines faster than the energy intensity may also be a sign that the rate of energy efficiency improvement has been constrained to be too low in most IAMs, even though these two issues are not directly related.	Given that emission levels ultimately have to reach zero under climate stabilization, carbon intensity of energy use will eventually have to go down to zero as well while there are limits on energy intensity reduction. A revised figure shows time-dependence of energy vs. carbon intensity improvements and indicates that energy intensity improvements typically dominate over the first few decades whereas carbon intensity improvement becomes the dominant contribution in the longer-term.
9884	6	46	18			The fact that the carbon intensity declines faster than the energy intensity may also be a sign that the rate of energy efficiency improvement has been constrained to be too low in most IAMs, even though these two issues are not directly related.	Given that emission levels ultimately have to reach zero under climate stabilization, carbon intensity of energy use will eventually have to go down to zero as well while there are limits on energy intensity reduction. A revised figure shows time-dependence of energy vs. carbon intensity improvements and indicates that energy intensity improvements typically dominate over the first few decades whereas carbon intensity improvement becomes the dominant contribution in the longer-term.
9885	6	47	1			It is noteworthy that the highest level of incremental energy reductions relative to the baseline scenario for just a couple of models is about 40% over 45 years, since the base year 2005. Clearly, that is less than 1% per year (it is 0.75%) on an incremental basis. Given the extensive literature on how rapidly incremental energy efficiency could be phased in if there was the political will to do so, 1% per year is very low. An incremental 2-3% per year could probably be achieved if need be, relative to the baseline. For many model runs the incremental level of efficiency improvement is only 20% over 45 years. These results clearly illustrate my concern that energy efficiency improvements have almost always, if not always, been overly constrained, and these facts ought to be discussed in a single section on scenario infeasibility.	It should be noted that energy and carbon intensity improvements were shown compared to baseline, i.e. in addition to the calculated energy intensity improvements another 1-2%/yr which are embedded in the baseline need to be added. In hte revised figure version an index compared to 2010 is shown which eliminated this baseline dependence. Some of the assessed scenarios feature energy intensity improvements of 2-3%/yr under stringent climate targets while others are more pessimistic.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8680	6	47	1			It is noteworthy that the highest level of incremental energy reductions relative to the baseline scenario for just a couple of models is about 40% over 45 years, since the base year 2005. Clearly, that is less than 1% per year (it is 0.75%) on an incremental basis. Given the extensive literature on how rapidly incremental energy efficiency could be phased in if there was the political will to do so, 1% per year is very low. An incremental 2-3% per year could probably be achieved if need be, relative to the baseline. For many model runs the incremental level of efficiency improvement is only 20% over 45 years. These results clearly illustrate my concern that energy efficiency improvements have almost always, if not always, been overly constrained, and these facts ought to be discussed in a single section on scenario infeasibility.	It should be noted that energy and carbon intensity improvements were shown compared to baseline, i.e. in addition to the calculated energy intensity improvements another 1-2%/yr which are embedded in the baseline need to be added. In hte revised figure version an index compared to 2010 is shown which eliminated this baseline dependence. Some of the assessed scenarios feature energy intensity improvements of 2-3%/yr under stringent climate targets while others are more pessimistic.
9887	6	47	15			This line cites various studies including EMF27 that the text relies on. However, it is my understanding that the EMF27 material is not yet published, and is not even accessible yet to the public. Yet, I thought that the IPCC assessments were supposed to include only results already in the scientific literature. Is there a problem, then, with including EMF27 results even though they might be interesting. One scenario study that was not included in this chapter or its references was the Tellus Institute study entitled "The Century Ahead: Searching for Sustainability", which is already published (Sustainability 2010, 2,2626-2651;doi:10.3390/su2082626). This study has four scenarios that represent an even greater range than most studies in terms of energy efficiency improvements and total final energy demand in 2100, etc. It also has two scenarios roughly consistent with RCP2.6. While it does not produce costs for scenarios, it has far more disaggregated information about all sectors of the economy than any other IAM reported on in this chapter, and thus its results would provide interesting contrasts and/or similarities with the results presented in Chapter 6.	The suggested publication will be considered for inclusion in the SOD. The reason for relying on results from the EMF27 study is that it includes a large set of integrated models which allows distilling robust elements of technology strategies. The EMF27 study has been completed in the meantime and submitted papers have been made available to the WGIII TSU for the SOD review.
8682	6	47	15			This line cites various studies including EMF27 that the text relies on. However, it is my understanding that the EMF27 material is not yet published, and is not even accessible yet to the public. Yet, I thought that the IPCC assessments were supposed to include only results already in the scientific literature. Is there a problem, then, with including EMF27 results even though they might be interesting. One scenario study that was not included in this chapter or its references was the Tellus Institute study entitled "The Century Ahead: Searching for Sustainability", which is already published (Sustainability 2010, 2,2626-2651;doi:10.3390/su2082626). This study has four scenarios that represent an even greater range than most studies in terms of energy efficiency improvements and total final energy demand in 2100, etc. It also has two scenarios roughly consistent with RCP2.6. While it does not produce costs for scenarios, it has far more disaggregated information about all sectors of the economy than any other IAM reported on in this chapter, and thus its results would provide interesting contrasts and/or similarities with the results presented in Chapter 6.	The suggested publication will be considered for inclusion in the SOD. The reason for relying on results from the EMF27 study is that it includes a large set of integrated models which allows distilling robust elements of technology strategies. The EMF27 study has been completed in the meantime and submitted papers have been made available to the WGIII TSU for the SOD review.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9888	6	47	22			The first three sentences here are to some extent repeats of prior material presented earlier in the chapter, and I have already commented on those. The first is obviously true and does not require research to support. The second needs to be either re-written to make it interesting, or eliminated as being unclear as what is really meant The third needs to be clarified also - does it simply mean that constraining the amount of mitigation technologies that can be deployed in one scenario relative to another raises the costs? If so, it is a mathematical truism, as I said before. And to the extent that it means that for any given model and input data set mitigation costs will rise, or become less negative, as the stringency of mitigation increases, that is also a mathematical truth and does not need to claim research support. If somethng else is intended it must be stated clearly.	Comment is noted and will be considered as the text is adjusted for the next draft.
8683	6	47	22			The first three sentences here are to some extent repeats of prior material presented earlier in the chapter, and I have already commented on those. The first is obviously true and does not require research to support. The second needs to be either re-written to make it interesting, or eliminated as being unclear as what is really meant The third needs to be clarified also - does it simply mean that constraining the amount of mitigation technologies that can be deployed in one scenario relative to another raises the costs? If so, it is a mathematical truism, as I said before. And to the extent that it means that for any given model and input data set mitigation costs will rise, or become less negative, as the stringency of mitigation increases, that is also a mathematical truth and does not need to claim research support. If somethng else is intended it must be stated clearly.	Comment is noted and will be considered as the text is adjusted for the next draft.
6501	6	47	25	47	27	This sentence should be left. Because it is described easily to understand that mitigation costs is doubled on average by more stringent CO2 equiv-target from 550 ppm to 450 ppm.	The section has been restructured, but the statement has been retained.
9886	6	47	6			Here infeasibility is discussed, again, for the third time. Please consolidate all these discussions into one section at the very end. Infeasibility is not a key issue in my opinion.	The discussion of infeasibility has been consolidated in the SOD.
13164	6	47	6	47	7	The title of section 6.3.7.3. is rather convoluted.	Section was reorganized and title changed
8681	6	47	6			Here infeasibility is discussed, again, I think for the third time. Please consolidate all these discussions into one section at the very end. Infeasibility is not a key issue in my opinion.	The discussion of infeasibility has been consolidated in the SOD.
6540	6	48				Explain the technology portfolio variations in Figure 6.29. and give a reference paper when available.	Due to space constraints Figure 6.29 has been removed. The technology variations will be explained in the caption of Figure 6.30.
7785	6	48		48		Premise in each energy use and other social conditions, on which the model analysis of figure 6.30 is based is uncertain. What factors did the scenarios take in account for each technology use?	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result.
7787	6	48		48		Premise in each energy use and other social conditions, on which the model analysis of figure 6.30 is based is uncertain. What factors did the scenarios take in account for each technology use?	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14403	6	48	1			Could clarify that going from a target of 450 to 550 implies a large proportionate increase in mitigation because we are already at 390	The issue of overshoot is discussed in Section 6.3.3.6 (SOD). In addtion, the metric used here is CO2-equivalent and includes contributions from gases and radiatively active substances other than CO2.
8684	6	48	1			Again, this first sentence is either a simple logical truth, or not clear.	Sentence has been removed.
9890	6	48	17			The influence of CCS on the overall cost results as discussed here make it all the more imperative that the CCS cost and performance input assumptions be presented somewhere in the text, again so that the readers can judge their reasonableness. Again, this report needs much GREATER TRANSPARENCY regarding the assumptions made by each modeling team. The should also mention that there is not yet a single major installation of CCS technology not associated with oil or gas fields, and there is a lot of public debate as to its feasibility and public acceptability. Even the well-known MIT report on CCS technologies of a few years ago is not optimistic.	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result. A more detailed discussion of the state and prospects of CCS development can be found in Section 7.5 of Chapter 7 on energy systems to which a cross-reference has been added.
13279	6	48	17	48	23	The importance of CCS has a 4th category: its application to carbon-intensive industry, especially those like cement and iron&steel that produce CO2 via chemical processes as well as fossil fuel combustion and that cannot therefore be largely decarbonised by using renewable or nuclear. Also, a further energy vector that can be produced in combination with CCS is synthetic natural gas.	The application of CCS in industry has been added to the list.
8686	6	48	17			The influence of CCS on the overall cost results as discussed here make it all the more imperative that the CCS cost and performance input assumptions be presented somewhere in the text, again so that the readers can judge their reasonableness. Again, this report needs much GREATER TRANSPARENCY regarding the assumptions made by each modeling team. The should also mention that there is not yet a single major installation of CCS technology not associated with oil or gas fields, and there is a lot of public debate as to its feasibility and public acceptability. Even the well-known MIT report on CCS technologies of a few years ago is not optimistic.	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result. A more detailed discussion of the state and prospects of CCS development can be found in Section 7.5 of Chapter 7 on energy systems to which a cross-reference has been added.
16027	6	48	17	48	23	Worldwide there is a big discussion and up to now very little real tested examples so this paragraph is to optimistic and blank out the risks	A more detailed discussion of the state and prospects of CCS development can be found in Section 7.5 of Chapter 7 on energy systems to which a cross-reference has been added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14404	6	48	20			Can you say something about the present dominant view of feasibility of CCS?	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result. A more detailed discussion of the state and prospects of CCS development can be found in Section 7.5 of Chapter 7 on energy systems to which a cross-reference has been added.
16722	6	48	23			Suggest insert at end of paragraph: "Likewise, models tend to show that if CCS is expensive, the resulting CO2 price is likely to be high, while if CCS is less expensive, the CO2 price will be lower.	A more detailed discussion of the state and prospects of CCS development can be found in Section 7.5 of Chapter 7 on energy systems to which a cross-reference has been added.
9889	6	48	6			I suggest that the text should make it very clear that the results reported in Figures 6.29 and 6.30 are extremely sensitive to the relative input cost assumptions for each technology listed. This is why I suggested earlier that a table of these key cost assumptions for these key technologies be included in the text so that the readers can form their own judgments as to the reasonableness of those assumptions. For example, I know that the nuclear powe input cost assumptions used by most modeling teams seem far lower than actual construction costs today, even corrected for inflation. Also, are their any costs assigned to efficiency in the models? If not, the text should explain that this is one reason by the high efficiency scenarios cost less.	The costs of technologies are only on set of indicators that have an influence on the costs of mitigation. The overall portfolio of available mitigation options - on the supply and demand side as well as in other sectors (e.g., agriculture) - and the substitutability between technologies tends to be more important than economic assumptions of individual technologies.
8685	6	48	6			I suggest that the text should make it very clear that the results reported in Figures 6.29 and 6.30 are extremely sensitive to the relative input cost assumptions for each technology listed. This is why I suggested earlier that a table of these key cost assumptions for these key technologies be included in the text so that the readers can form their own judgments as to the reasonableness of those assumptions. For example, I know that the nuclear powe input cost assumptions used by most modeling teams seem far lower than actual construction costs today, even corrected for inflation. Also, are their any costs assigned to efficiency in the models? If not, the text should explain that this is one reason by the high efficiency scenarios cost less.	The costs of technologies are only on set of indicators that have an influence on the costs of mitigation. The overall portfolio of available mitigation options - on the supply and demand side as well as in other sectors (e.g., agriculture) - and the substitutability between technologies tends to be more important than economic assumptions of individual technologies.
6506	6	48	6	48	10	Figure 6.29 should be left. Because it is illustrated easily to understand that mitigation costs is doubled on average by more stringent CO2 equiv-target from 550 ppm to 450 ppm.	Figure 6.29 was removed due to space constraints, but another figure that illustrates the change in costs between 550 and 450 ppm targets has been added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11261	6	48				I am completely missing the discussion of the EERE scenario in Fig 6.29 and 6.30. It is very interesting that despite the fact that you refrain from CCS and nuclear, you can lower the costs substantially. For the 550ppm scenario the effect of refraining from CCS is as large as the effect of the EERE scenario. This should be elaborated further, this is an extremely interesting result.	Comment is noted and will be considered as the text is adjusted for the next draft.
11754	6	49				Clarification is needed why the mitigation costs with no nuclear case are almost same in Figure 6.30. If the reason comes from conditon of the Model, such kind of remark should be added in order for readers to recognize.	The figure shows the cost increase of technology constrained scenarios relative to scenarios with a default technology portfolio. Therefore, the similarity of the ranges presented for nuclear indicates that the technology does not increase in importance when moving from a 550 to a 450 CO2-equivalent target.
9577	6	49				Two charts of (a) and (b) look strange relationship as mitigation costs of no nuclear apear to be a similar range in (a) and (b). Please, provide the reason in the text.	The figure shows the cost increase of technology constrained scenarios relative to scenarios with a default technology portfolio. Therefore, the similarity of the ranges presented for nuclear indicates that the technology does not increase in importance when moving from a 550 to a 450 CO2-equivalent target.
6759	6	49				The treatment of nuclear power in this model analysis should be specified in Figure 6.30. □	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result, but the specification of the "no nuclear" case will be explained.
10650	6	49				The 450 ppm No Nuclear case is similar to the 550 ppm No Nuclear one. It is strange. There may be some conditions on the cases. Please provide the remarks on the conditions.	The figure shows the cost increase of technology constrained scenarios relative to scenarios with a default technology portfolio. Therefore, the similarity of the ranges presented for nuclear indicates that the technology does not increase in importance when moving from a 550 to a 450 CO2-equivalent target.
5870	6	49				Please clarify: The numbers are shares of 7 (or more) scenarios each and not numbers of models using reduced technology portfolio scenarios and - of these - number of feasible scenarios? Else the number of feasible scenarios exceeds the number of reduced technology portfolio scenarios which does not make sense.	Figure 6.29 has been removed due to space constraints. However, an explanation of the numbers at the bottom of the figures has been added to the figure caption of Figure 6.30.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9987	6	49				In this figure, there should be an explanation about the reason why the ratios of nuclear power generation are same in the 550 ppm case and the 450 ppm case. It seems that the capacity and/or generation of the nuclear is intentionally limited and set as the same in both cases. Many assessment models assume the limitation of nuclear power capacity and/or generations considering the public acceptability. It seems that the results are based on this assumption. If so, the results underestimate the contribution of nuclear power in terms of mitigation costs.	The figure shows the cost increase of technology constrained scenarios relative to scenarios with a default technology portfolio. Therefore, the similarity of the ranges presented for nuclear indicates that the technology does not increase in importance when moving from a 550 to a 450 CO2-equivalent target.
9891	6	49	14			Finally, here it is stated that the costs for implementing energy efficiency have not been taken into account "by all models". It should say which models do take it into account, and it should try to estimate the approximate size of the certain "downward bias" this creates in many results presented in this chapter. This major omission must be clearly labeled and identified in each relevant section of this chapter. It is not a "bias", it is a weakness in the structure of the models that omit these types of costs.	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result. The point here is that the cost estimate of the low energy intensity case might not be completely comparable with the cost estimates of supply side technology variations.
8687	6	49	14			Finally, here it is stated that the costs for implementing energy efficiency have not been taken into account "by all models". It should say which models do take it into account, and it should try to estimate the approximate size of the certain "downward bias" this creates in many results presented in this chapter. This major omission must be clearly labeled and identified in each relevant section of this chapter. It is not a "bias", it is a weakness in the structure of the models that omit these types of costs.	Given space constraints of IPCC reports it will unfortunately not be possible to describe the underlying assumptions of each and every result. The point here is that the cost estimate of the low energy intensity case might not be completely comparable with the cost estimates of supply side technology variations.
13280	6	49	17	49	23	A wide range of energy system models include the possibility to switch parts of the transport system to electricity and/or hydrogen. Rather than citing one integrated model that does incorporate this possibility, would it not be more sensible to say that the rest tend to overstate the costs of mitigation because they ignore such important options?	Other studies that have explicitly explored the relevance of a transition to electricity/hydrogen in transport have been added.
8688	6	49	18			How do demand-side efficiency measures facilitate the use of low carbon fuels? This point is not clear, and the explanation offered is not clear. Generally, the two issues seem to be independent of each other.	The statement does not refer to efficiency measures, but to demand-side measures more generally. This includes fuel switching which is one important optionto facilitate the use of low-carbon fuels such as electricity. In SOD this will be clarified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6693	6	49	2			With its cost-effectiveness, Nuclear energy will play an important role in reducing mitigation costs than CCS and renewable energies. However, in this figure, an increase in mitigation costs due to constraints of technology availability of nuclear enegy is underestimated, compared with the other energies. It is probably because these estimations are based on the assumptions for the analysis in many models. Many models assume the exogenous scenario or limitation of nuclear power capacity or generations considering the public acceptability. Given such assumptions, the role of nuclear energy to mitigation costs tends to be underestimated. The nuclear energy capacities are inherently limited in the original modeling set-up, which leads to little change in nuclear power generation under different levels of emission reductions. Such assumptions lack a scientific basis and are often determined by modelers on an ad-hoc basis. They bring underestimation of the benefit of nuclear energy. So, the additional explanatory remarks of the figure discussed above should be added in the body text in order to avoid misunderstandings.	Those options that are largely confined to the electricity sector (e.g., wind, solar and nuclear energy) tend to show a lower technology value, because there are a number of low-carbon electricity supply options available that can generally substitute each other.
9892	6	49	24			Again, infeasibility is discussed from lines 24-32. Consolidate and move to end.	The discussion of infeasibility has been consolidated in the SOD.
8689	6	49	24			Again, infeasibility is discussed from lines 24-32. Consolidate and move to end.	The discussion of infeasibility has been consolidated in the SOD.
6503	6	49	29	49	32	This sentence should be left. Because it is described briefly that strict CO2 equiv-target does not produce scenarios with limited technology portfolio.	The section has been restructured, but the statement has been retained.
10994	6	49				In this figure, mitigation cost is not so different from 550 ppm to 450 ppm in the case of no nuclear. Why is such a result conducted? In comparison with other means, nuclear energy is seemed to be underestimated for mitigation.	Figure 6.30 shows the relative increase of mitigation costs in the absence of specific technologies compared to a case with the full (model-specific) technology portfolio. Given that costs in the full portfolio case increase from 550 to 450 ppm, this means that the cost increase in the "no nuclear" case increase more under the 450 ppm target than under the 550 ppm target.
6262	6	5		11		While this introductory material is well written, there is substantial repetition of text as well as ideas. Streamlining this introductory material (the Executive Summary, Section 6.1 and Section 6.2) is one way to reduce the number of pages in this chapter.	Editorial.
6265	6	5		21		There is a real dearth of citations to the peer reviewed literature in this early material. There is certainly more than one paper that looks at the increased flexibility that is had from including multiple gases rather than just CO2 (there are too many examples to try and list them all here). An important aspect of AR5 is to survey the existing peer reviewed literature. It is clear that a tremendous effort has gone into putting together the database that CHapter 6 uses but there still is a need to have citations to specific aspects of the peer reviewed literature in the text when a specific point is being made. In the FOD, it seems that the "reference" for much of what is stated is the database. I'm not sure that is sufficient and this is something that the authors of this chapter might want to address as they prepare the SOD.	Accepted. The referencing will continue to be enhanced as the process moves forward.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7849	6	5		84		It is noted that this chapter does not consider the amount of fossil fuels that have already been explorated and which already own e.g. to a company. These resources correspond to significant amount of economic value and any scenario has a significant impact on its market value. The potential loss of market value should be considered as a major driver of policy decisions that finally will translate into the actual emission pathway.	Rejected. This is a topic for the finance chapter.
14388	6	5	1			“many pathways” is a bit at odds with the seeming pessimismistic tone of Chapter 1, which comes close to saying 2° is impossible	Noted.
8056	6	5	1	8	21	In the Ex Summary I would like to see more what was said on linkages of mitigation and adaptation (and or knowledge gaps): Compare with page 27, lines 43 - 46	Rejected. This chapter is focused on mitigation, not on adaptation.
8057	6	5	1	8	21	In the Ex Summary I miss the clear language on the necessary great transformation like in page 83, line 45 to page 84 to line 5: 'Within this context, research indicates that efforts to meet a 2.6 W/m2 will be challenging under all strategies, but extraordinarily challenging without the option to overshoot this goal temporarily, substantial, near-term global emissions reduction, coordinated action to achieve these reductions, and a full complement of available technology options including CCS and nuclear power. Indeed, studies indicate a global emissions peak prior to 2020 to meet this goal, with associated dramatic near-term transformations in the energy system and social and institutional infrastructure for producing and consuming energy.' This fits to page 8, line 7	Noted. The language to describe the requiremments to meet long-term goals is being refined. At the same time, efforts are being made to strike value judgments like "extremely challenging" from the text.
8058	6	5	1	8	21	Also 'At the same time, these idealized circumstances are unlikely to materialize. Studies indicate that delays in global action or fragmented action regimes in which mitigation is not undertaken where and when it is least expensive or in which policy structures are not designed to minimize costs can all increase costs dramatically, more than XX% in some circumstances' (p85, line 9 - 12) is worth for being in the Executive Summary.	Rejected. The subjective assessment of what policy structures are or are not likely will be removed from the chapter.
14031	6	5	10			Some of these other societal priorities would also be economic growth and job security	Noted.
14032	6	5	12			Add "cultural change"	Noted.
8612	6	5	13			American pioneers	Noted but not understood.
4767	6	5	17	5	27	Yes I agree, but the difficulty is to monetise (give an economic value) to those services (first thing is to recognise all those services, and second to monetise them)	Noted.
6094	6	5	18	21		Executive summary is excellent. I hope other chapter follow the lead.	Noted.
9825	6	5	19			Here, and throughout the chapter, the economic costs of climate change mitigation are refered to. However, the possibility of net benefits, not costs, must also be included as an appropriate balanced approach. I do not know why the authors think that net costs is the only possibility, even though all runs of existing IAMs might yield that result. They yield that result, in part, because reference case scenarios may not have been run with fossil fuel prices sufficiently above those in the mitigation scenarios, due to the higher prices that might result from higher demand for fossil fuels in the reference cases. Please please rewrite all the economic cost sections acknowledging the possibility of net benefits resulting from mitigation compared to reference cases. Net benefits could also result in renewable energy was much cheaper than fossil-fuel energy, etc.	Noted. The notion of negative costs will be mentioned in the SOD.
8613	6	5	19			Here, and throughout the chapter, the economic costs of climate change mitigation are refered to. However, the possibility of net benefits, not costs, must also be included as an appropriate balanced approach. I do not know why the authors think that net costs is the only possibility, even though all runs of existing IAMs might yield that result. They yield that result, in part, because reference case scenarios may not have been run with fossil fuel prices sufficiently above those in the mitigation scenarios, due to the higher prices that might result from higher demand for fossil fuels in the reference cases. Please please rewrite all the economic cost sections acknowledging the possibility of net benefits resulting from mitigation compared to reference cases. Net benefits could also result in renewable energy was much cheaper than fossil-fuel energy, etc.	Please see the response to comment 9825, which appears to be a duplicate of this comment, despite being submitted by another reviewer.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4766	6	5	2	5	16	I fully support this statement, the target is important, but the path to ii is even more	Noted. At the same time, the ES is being revised for the SOD, and this statement may be removed for space and left only in the introduction.
2254	6	5	2	5	3	Since there is no evidence that increases in greenhouse gases have a harnful effect on the climate the whole exercise of this chapter appears to be futile, inless there are other "abthropogebic" effects which are considered "dangerous"	Please see WGI and WGII
3628	6	5	2	5	3	Please refer to Article 2 of UNFCCC.	Noted. This phrase may not longer be found in the ES in the new revisions. Regardless, whether it remains or a similar statement remains in the introduction, we will no longer include a reference to dangerous anthropogenic interference with the climate.
7847	6	5	2	5	3	It would be very interesting to know the basis of this statement made in the first sentence. Such statement is only possible if there is a common understanding about that level of climate change that does not yet represent a dangerous interference with the climate system. For the time being there is no real political agreement on such level, expressed as temperature increase compared to earlier levels. The following wording is suggested: There are many transformation pathways to stabilization of greenhouse gas concentrations at a given level.	Noted. This phrase may not longer be found in the ES in the new revisions. Regardless, whether it remains or a similar statement remains in the introduction, we will no longer include a reference to dangerous anthropogenic interference with the climate.
11367	6	5	22	5	22	The term 'good decisions' is a bit vague and probably too open. Please reconsider that and specify.	Noted.
12622	6	5	26	5	27	I see no reason to single out any technologies here. All technologies include trade offs, CCS, Nuclear, Wind, Solar, etc.	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
12665	6	5	26	5	27	I see no reason to single out any technologies here. All technologies include trade offs, CCS, Nuclear, Wind, Solar, etc.	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
11744	6	5	26	5	27	Other low carbon technologies like wind and geothiermal have also environmental problems to resolve and CCS isn't only for coal-fired power. It is strange only nuclear and coal-fired CCS are included as examples of other environmental factor. [nuclear power] should be amended to [low carbon technologies] and [coal-fired] should be deleted.	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
9563	6	5	26			Please, delete examples of nuclear and CCS, or add examples of wind power and geothermal as they involve bird strikes (wind power) and sources of mercury contamination (geothermal power).	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
9564	6	5	27			Please, remove coal-fired from coal-fired CCS as we need any types of CCS in terms of negative and positive emissions.	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
7848	6	5	27			The term "coal-fired CCS" might be technical jargon but should be substituted by a more complete term such as: coal-fired power plant with CCS	Editorial
13120	6	5	28	5	29	The wording is a bit courageous. Surely not ALL countries MUST bring their emissions "toward zero" for meeting ANY stabilization goal? Reformulate.	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4188	6	5	28	5	29	The expression ".. all countries must ultimately bring their emissions toward zero to meet any stabilization goal." seemes to be exaggerated, since equilibrium still allows some emission.	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.
7390	6	5	28	5	29	This is demonstrably incorrect for short-lived gases, where constant emissions still result in stabilisation. It is only true for very long-lived gases. As this is a very policy relevant issue, please make clear that abatement of short-lived gases helps reduce costs but is not a physical necessity, whereas reduction to zero of long-lived gases, particularly CO2, is an absolute physical necessity to meet stabilisation goals.	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.
14389	6	5	29			Bring “toward zero”?? My figure is 1.4 tCO2 per person per year	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.
3070	6	5	29			“all countries must ultimately bring their emissions toward zero to meet any stabilization goal”. This is wrong scientifically (if GHG have a constant finite atmospheric residence time they will stabilize at some elevated level for any emission rate). It is also clearly a fantasy---no country is going to give up vehicular or air travel, for example, or heat all its buildings with electricity (very expensive, even if nuclear power comes back into fashion).	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.
6535	6	5	29			Replace "bring their emission toward zero" with e.g. "reduce their emissions significantly" in accordance with AR4 WG1 Report Figure 10.21, or give a reference paper.	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.
3071	6	5	3			“dangerous anthropogenic interference with the climate” is advocacy, not science, and ignores the fact that the extensive scientific effort devoted to climate modeling has not been matched by any significant effort devoted to determining whether warming or climate change will, on balance, help or harm humanity. The Medieval Climate Maximum was a time of prosperity, at least in northern Europe, and the Little Ice Age a disaster. This may not be extrapolatable to modern anthropogenic warming, but the question has hardly been asked, much less answered.	Noted. This phrase may not longer be found in the ES in the new revisions. Regardless, whether it remains or a similar statement remains in the introduction, we will no longer include a reference to dangerous anthropogenic interference with the climate.
9826	6	5	30			Not all countries must undertake substantial reductions in emissions, some can maintain their level or even increase their level in a decent way.	Noted. This phrase may not remain in the SOD. If it remains, it will be made more clear.
13121	6	5	30	5	32	I'd suggest moving this conclusion later in the paragraph - currently mitigation quantities and costs are brought up before they are properly defined (i.e. only after the conclusion it's explained that costs and quantities are calculated against a baseline, not, for example, the base year)	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD.
14033	6	5	30		42	I wonder how useful it is to distinguish between developing and developed countries when talking about where the largest cuts in emissions will need to take place in the future. If this is brought into the executive summary and is meant to inform the need for coordinated international action to meet global goals, there should be more discussion on where the largest emissions will take place, for example in Asia.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11416	6	5	31	5	35	The assertion that developing countries will have to undertake greater levels of emission reductions because their emissions are projected to be larger than those of developed countries over the coming century needs to be explained more clearly in terms of what the assumptions are underlying such assertion. Absent a clear explanation of the assumptions for this assertion, such a bare assertion could be used in a non-scientific and political way in order to push specific policy agendas or approaches in the context of international policymaking discussions and negotiations on climate change that could effectively absolve developed countries of any further mitigation commitments and increase the pressure on developing countries to undertake increased mitigation actions.	Noted. The basis for this assertion, continued economic growth in the developing countries, is discussed in greater length in the chapter.
14390	6	5	33			Should clarify that cutting emissions to a low uniform level would be smaller percent cut for most developing countries. (India is currently at about 1tCO2 per capita.)	Noted.
13122	6	5	43	6	5	The conclusion here seems to suggest that the scenario results can be used as proof for when emissions need to peak. I don't find this convincing, as this depends completely on the assumptions that have been used when constructing the models (which is acknowledged in the table caption that follows, but not in the text). For example, if a break through for cheap air capture technologies was assumed in the models, the peak could presumably be later. At the very least I would like to see this conclusion supported by a purely carbon budget related argument before I would call the evidence "robust".	Noted. An attempt is being made to distinguish between scenarios with and without negative emisisons technologies and then with different levels of overshoot and delayed action. Regardless, this table will no longer be in the ES because of space constraints.
11368	6	5	43	5	47	This statement needs careful reading . It should be reformulated or cut into several sentences to achieve better reading and reasoning.	Accepted. Sentence needs work. Regardless, the ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD.
14391	6	5	48			It is annoying when this chapter frequently uses the W/m2 metric rather than the more familiar degrees C or ppm.	Accepted. The chapter is moving to the ppmv CO2-e notation.
9824	6	5	9			also lifestyles and the moral values we hold should be included in the list of choices we must make	Noted.
8611	6	5	9			"the treatment of land use" sounds like a modeling issue - perhaps you mean "how land is used"; also lifestyles and the moral values we hold should be included in the list of choices we must make	Noted.
11243	6	5				The executive summary is so far only an introduction how to read scenarios, but there is nearly no content. Fig 6.ES.1 e.g. does not contain an important message, it is trivial (or did I miss something?). I also find Table 6.ES.1 not very informative because it is not clear if this is meant for the idealized scenarios or for a mean over all scenarios.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD.
11244	6	5				A historical context is missing: there is no reference to AR4 and even not to the SRREN. It would be interesting to know what happened with the models since AR4? How have models developed (e.g. including now BECCS)? What has been learned from the SRREN? What is new in the political discussion that chp. 6 should be able to answer?	Noted. We intend to provide a greater link to AR4 in the next version of the ES. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD.
7712	6	5		7		Too much repetetion for describing why this chapter is not complete. The senario models are mostly too premature and they need much time to be established. All the descriptions on 'uncertainty of the models' are very much troublesome for readers. Reconsiderations for the structure would be highly appreciated.	Noted.
16683	6	5		8		Should bring forward and highlight the point made on page 36 of chapter 6 about the benefits of policies that put a price on CO2 emissions -- they provide by far the most efficient, least cost means to reduce emissions. Policymakers should be reminded of this in the context of transformation of the energy system in the executive summary.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10979	6	5	19	5	22	What's the role of "measures of macro-economic costs such as GDP losses or changes in total personal consumption" if it were "far from the only characteristics about transition pathways that matter for making good decisions"? Is it a supplementary factor?	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD.
12608	6	5	1	8	21	On the Executive Summary. Woud be good to change to a different letter font the principal message or bullets.	Editorial
9893	6	50	1			It seems to me that land-use issues, and how they interact with other mitigation issues, are so complex and so important, that they deserve more extensive treatment. More material should be presented and the entire topic should be explained relative to how land-use is taken into account in some of the most important IAMs, including the Tellus Institute scenario model Polestar. (See reference above.) Land-use issues should be integrated into each sub-section of this chapter where appropriate. For example, land-use issues should be discussed when discussing model structure, input assumptions, results, policies, etc.	Author team agrees that land-use needs to be better integrated throughout chapter, e.g., reference emissions, model descriptions. Will consider the Polestar publication and how it fits into the chapter.
8690	6	50	1			It seems to me that land-use issues, and how they interact with other mitigation issues, are so complex and so important, that they deserve more extensive treatment. More material should be presented and the entire topic should be explained relative to how land-use is taken into account in some of the most important IAMs, including the Tellus Institute scenario model Polestar. (See reference above.) Land-use issues should be integrated into each sub-section of this chapter where appropriate. For example, land-use issues should be discussed when discussing model structure, input assumptions, results, policies, etc.	Same as previous comment.
7468	6	50	16	50	19	“Uncertainty about land-related baseline CO2 emissions and sequestration is significant historically (Houghton et al., 2012; Pan et al, 2011) and in projections. The latest baseline projections for land related CO2 emissions show an enormous range across integrated assessment models, which begins with historical years (Figure 6.31)”. Some of the annual et primary production (NPP), an estimated 53.2 Gt C, may be sequestered. However, the annual use of NPP for energy and non-energy purposes is of the order of 3.5 Gt C, (see general comments in Ch.7). Therefore, there is a considerable surplus of the annual growth of biomass. Thus, the various lines in figure 6.31 may be an over estimate of CO2 emissions from biomass.	Fig 6.31 is supposed to be projections of NET LUCF emissions. We are verifying that that is the case. All of the projections should be either net or gross, and we prefer for the former for this purpose.
14405	6	50	26			Explain the sink. Adoption of ambitious afforestation programs?	Comment pertains to Fig 6.31. We clarify that in the long-run a terrestrial sink is projected by many models.
6916	6	50	2			Refer to WGI AR5, Chapter 6, for mechanisms and quantitative assessement of sources/sinks from carbon and other biogechemical cycles.	Reviewing WG1 AR5 Ch6 and will cite accordingly.
14687	6	502	24	502	26	It is worth qualifying this sentence with the preface "if it was to be effective in practice, the net effect would be to accelerate...etc....".	We could not figure out which part of the chapter this comment is referring to. Neither in the single chapter PDF version nor in the full FOD PDF file is there a page 502 that is part of Ch.6.
14688	6	502	28	502	30	It is important that the boundaries of this cost estimate are made explicit, i.e. does this simply include the cost of the iron, or also the cost of transporting, deploying, monitoring imacts and effectiveness, etc. Costs depend on so many factors that it is important not to prejudge cost of what remains an abstract concept.	We could not figure out which part of the chapter this comment is referring to. Neither in the single chapter PDF version nor in the full FOD PDF file is there a page 502 that is part of Ch.6.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14689	6	502	43	502	43	It is probably worth noting here that ocean fertilisation activities are now controlled under the London Convention/London Protocol Resolutions LC-LP.1(2008), which disallows all ocean fertilization activities other then legitimate scientific research, and LC-LP.2(2010), which established the assessment framework to determine whether proposed ocean fertilization activities constitute legitimate scientific research (http://www.imo.org/blast/mainframemenu.asp?topic_id=1969). It is also worth noting that , because of concerns over impacts, the direct disposal of CO2 into the water column or on the seabed has been prohibitted in some regions, most notably the North-East Atlantic region under the OSPAR Convention (OSPAR Decision 2007/1 to Prohibit the Storage of Carbon Dioxide Streams in the Water Column or on the Sea-bed, http://www.ucl.ac.uk/cclp/pdf/OSPAR2007-Annex-5.pdf).	We could not figure out which part of the chapter this comment is referring to. Neither in the single chapter PDF version nor in the full FOD PDF file is there a page 502 that is part of Ch.6.
14690	6	502	46	503	1	Proposals for alkalinity management do not strinctly accelerate weathering but rather aim to mimic the effect of such enhanced weathering through artificial addition. In addition to concerns regarding impacts of mining, processing and trasmporting on land, the Expert Group report for the CBD SBSTTA on "IMPACTS OF CLIMATE-RELATED GEOENGINEERING ON BIOLOGICAL DIVERSITY" (UNEP/CBD/SBSTTA/16/INF/28, http://www.cbd.int/doc/meetings/sbstta/sbstta-16/information/sbstta-16-inf-28-en.pdf) noted that "While the theoretical chemistry of the processes of enhancing ocean alkalinity is relatively straightforward, the impacts on those processes on biodiversity (if the technique were to be deployed) are much more uncertain. In particular, the biological effects of temporarily enhanced Ca2+ ions and dissolved inorganic carbon are not adequately known". It may be worth including some reference to this	Risks are already mentioned and we believe this is more detailed than can be accommodated in a space allocated.
14686	6	502	4			This entire section would benefit significantly from greater consideration of potential adverse impacts of commonly proposed geoengineering methods, drawing perhaps on the recent report of the Expert Working Group on impacts of geoengineering on biodiversity under the CBD (http://www.cbd.int/doc/meetings/sbstta/sbstta-16/information/sbstta-16-inf-28-en.pdf)	I think this is a reasonable suggestion, one way to deal with it would be to include a table of adverse impacts along with potential benefits.
14691	6	503	17	503	21	There are also significant concerns regarding the potential environmental impacts of the most commonly used or proposed amine-based capture chemicals, e.g. Padurean, A., Cormos, C.-C., Cormos, A.-M., Agachi, P.-S. (2011) Multicriterial analysis of post-combustion carbon dioxide capture using alkanolamines. International Journal of Greenhouse Gas Control 5: 676-685 .	There are many proposed approaches to direct carbon capture which are only briefly mentioned in this section. Section 6.5.3 covers these in more detail but does not raise this particular issue.
14692	6	503	23	503	23	It is unwise at this stage to state in an unqualified way that SRM has a role in shaping climate policy as it is not clear that it would be effective in any manner, or acceptable as a policy approach. It may be expected to act relatively quickly in reducing solar radiation reaching the Earth's surface, but the speed, uniformity and effectiveness of action remains unknown.	The existence of geoengineering may shape policy outcomes even though it is uncertain and may not be used.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14693	6	503	26	503	27	once again this is a highly theoretical treatment and should be explicitly so - it cannot yet be said that SRM 'CAN' temporarily and imperfectly maskclimate change - these are, of course, theoretical modelled prediction,s not empirical observations.	The literature on solar geoengineering now spans many hundreds of papers published over many decades. That literature in turn rests on a body of scientific knowledge of climate that is substantially the same as the body required to understand the climate impacts of greenhouse gases and aerosols. This understanding in turn rests observations as well as theory. It is certainly true that the literature on geoengineering is smaller than the total literature on many other topics, but cannot argue that geoengineering is pure speculation without being forced to concede (falsely) that discussion of greenhouse gas driven climate change is pure speculation.
14694	6	503	34	503	34	In fact, there is little evidence that public understanding of SRM is growing 'rapidly' - without stressing that public awareness understanding is starting from (and remains at) a very low baseline, this statement could be misinterpreted as implying commen knowledge and perhaps even widespread acceptance.	We will edit the text to ensure that we are not implying anything about acceptance of SRM.
14696	6	504	21	504	29	This paragraph is also currently quite unclear in significant parts and will need to be reviewed in detail once the text has been redrafted and the meaning is clear	text completely revised, comment no longer applies
14695	6	504	6	504	16	This paragraph is currently very unclear, with some sentences being very long and others incomplete. It also implies once again that SRM is demonstrably able to compensate for changes in temperature or precipitation, without making clear that these are 'in principle' theoretical statements based on limited modelling. In some cases, the model outputs have been tuned by specfici inputs in order to compensate, and it is vital that model parameters and outputs are also distinguished clearly.	The literature on solar geoengineering now spans many hundreds of papers published over many decades. That literature in turn rests on a body of scientific knowledge of climate that is substantially the same as the body required to understand the climate impacts of greenhouse gases and aerosols. This understanding in turn rests observations as well as theory. It is certainly true that the literature on geoengineering is smaller than the total literature on many other topics, but cannot argue that geoengineering is pure speculation without being forced to concede (falsely) that discussion of greenhouse gas driven climate change is pure speculation.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7469	6	51	13	51	16	"GHG mitigation opportunities in land are of one of three types: emissions reductions, terrestrial carbon stock enhancement, or biomass displacement of fossil-fuel based energy. Bio-based products are also a possibility, but one not yet modeled. For a more complete discussion of mitigation technologies, as well as mitigation supply potential, see Chapter 11". The accessible NPP is at least 13.4 Gt C. Thus with improved management , some of this annual growth of biomass could be sequestrated and/or used for energy and non-energy purposes. Most if not used, returns to the atmosphere.	No reference provided. However, there were similar comments on chapter 11. The results reflect the economic costs of mitigation. Technical potential is much larger, but is not cost-effective, as cheaper mitigation options across the economy are available.
3380	6	51	17	51	25	The numbers on bioenergy mitigation potential reported here are high. Here is the following concern: A number of carbon GHG dynamics, e.g. carbon stock dynamics, soil emissions, N20 emissions (high uncertainty), ILUC, but also non-GHG issues, such as albedo tend to make assessment of the global warming impact of bioenergy deployment quite complex and challenging. I am relatively sure that most IAM models used for producing these numbers are relatively ignorant of these effects (see Creutzig et al., 2012). Sometimes, effects point to a positive effects on the global warming impact. Mostly, however, these dynamics seem to compromise the mitigation potential of bioenergy. These dynamics are also relevant for advanced bioenergy sources, e.g. energy crops, but also forest residue use. etc. I am not saying that such high numbers as reported here are not possible. I am saying that the models used are focussing on one set of scenarios, mostly optimistic, and that under plausible other model assumptions, the potential could be significantly lower. A note of caution when interpreting the numbers presented here would hence be appropriate from my perspective. F. Creutzig, A. Popp, R. Plevin, G. Luderer, J. Minx, O. Edenhofer (2012) Reconciling top-down and bottom-up modeling on future bioenergy deployment. Nature Climate Change 2: 320-327	Agree that it is important to properly characterize the state of modeling and caution about potential bias in results. Existing text does to a degree, but will consider the citation provided to incorporate missing aspects.
9894	6	51	18			If such high proportions are correct, then this implies that land-use issues and modeling must be fully integrated in all sections of chapter 6, as I suggested above.	See previous reply line 766.
8691	6	51	18			If such high proportions are correct, then this implies that land-use issues and modeling must be fully integrated in all sections of chapter 6, as I suggested above.	Same as previous comment.
7470	6	51	26	51	30	"More generally, transformation pathway studies have produced total global land-use CO2 emissions reductions of up to 5 and 6 GtCO2/year [1.4 – 1.6 Gt C] in 2030 and 2050 respectively (Fisher et al., 2007); L. Clarke 27 et al., 2009), with up to 10 GtCO2/year [2.7 Gt C] having also been estimated (Wise et al., 2009), in scenarios in which terrestrial carbon is subject to the same immediate and global price as fossil and industrial emissions". As stated above, the accessible NPP is at least 13.4 Gt C, with present use estimated to be 3.5 Gt C. Therefore there is a considerable surplus of accessible annual growth of biomass, to more than satisfy the above forecasts. Most of the traded biomass is very competitive when compared to fossil fuels. In fact fossil fuels and electricity are subsidized in many countries.	See previous reply line 770.
13167	6	51	36	51	38	This message doesn't come through very strong from the figure and I wouldn't add a reference to it in this sentence. It would be much more appropriate later, for example for the sentence on lines 39 and 40 ("However…")	Good suggestion.
18634	6	52				Page 52 contains an interesting reasoning on the need for policy coordination (but shouldn't that be discussed in the policy chapters?)	It is here because it affects cost and net effectiveness of land based mitigation. Coordination should be in policy chapter as well.
9895	6	52	19	52	29	The same can be said for bioenergy and it´s interaction with land-use	Couldn't figure out the meaning of this comment.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
7471	6	52	19	52	23	"To understand bioenergy's transformation role, it is important to understand bioenergy's role within the energy system. The research results surveyed in (Rose et al., 2012) found bioenergy contributing up to 15% of cumulative primary energy over the century during stabilization. Figure 6.33 shows more recent annual results, where bioenergy is projected to provide 20 to 250 EJ in 2050 (10 to 30% 22 of total primary energy) and 10 – 330 EJ in 2100 (20 to over 40%) for immediate global action scenarios". While bioenergy may contribute up to 15% of the primary energy system, the accessible annual growth of biomass is of the order of 500-515 EJ, and the total terrestrial NPP is an estimated 2000 EJ. The accessible NPP is much greater than the figures estimated on lines 19-23! Again, the estimates in Figure 6.33 are on the low side.	See previous reply line 770. Note that cost is a consideration. So, while there may be substantial NPP or EJ of biomass available it may be costly to access and to convert (not to mention net emissions and coordination issues).
6402	6	52	37	52	40	All of these acronyms or abbreviations are confusing. The reader doesn't know what they refer to, and what they mean. When I got to this, I naturally wanted to gloss over this and skip this part.	Thank you for pointing this out.
5871	6	53				Not legible, enhance / rework or delete. 3 x 15 colums along the x-axis is too much!	Agree. A different figure needed.
6403	6	53	15	53	15	Abbreviation of BioCCS is inconsistent with other abbreviations in the chapter (BECS, BECCS)	Standardizing to BECCS
9587	6	53	15	53	17	Please, describe the reality of BioCCS here as it may have limitation to deploy and uncertainty as follows; Rhodes and Keith in a 2008 peer-reviewed commentary on biomass with capture noted that while the high end of estimates for potential biomass availability support the view that biomass could provide the central mechanism for managing global climate and energy challenges, it is doubtful because [1] of the deep uncertainty in the feedstock supply estimates; the environmental implications of maximizing production; the complex social and ethical issues arising from the required re-organization of global land use; and the potentially high costs of such a strategy. They further note that [2] relatively large allocations of land in the developing world would be required to support the scales of bio-energy development implied by globally-aggressive biomass-based strategies. For example, land availability estimates indicate that 84% of arable land not in commercial use is in tropical regions of the world. Local food production capacity, which likely represents a more immediate concern in the developing world than carbon emissions, could be displaced. More generally, rural populations could be forced to adapt to radically changed local environments, including environmental consequences from large-scale biomass production. The notion that these disruptions should be absorbed by the developing world in order to mitigate carbon emissions in industrialized nations raises complex ethical issues of "biomass justice". [1] J.S. Rhodes and D.W. Keith (2008) Biomass with capture: negative emissions within social and environmental constraints: an editorial comment, Climatic Change, 87, p. 323, lines 9-14. [2] J.S. Rhodes and D.W. Keith (2008) Biomass with capture: negative emissions within social and environmental constraints: an editorial comment, Climatic Change, 87, p. 323, lines 31-41.	Need a clear statement that all indications are that realities of bioenergy are complex and challenging. Sustainable bioenergy, if it exists, has yet to be identified. Cross-referencing bioenergy x-cut as well.
13170	6	53	19	53	21	This should also be mentioned on page 48, lines 17-23. Currently bioCCS is mentioned as one of three reasons why CCS is important for 450 ppmCO2eq, but this text here suggests that the three reasons mentioned are unlikely to be equally significant.	Thank you. Coordinating on text across sections.
7472	6	53	28	43	31	"There are significant challenges to accessing the potential estimated above. Among other things, there are large fundamental historical scientific uncertainties about terrestrial carbon stocks and fluxes (e.g., (Henry et al., 2011); Houghton et al., 2012) that combined with uncertainty about economic behavior, complicate estimation of mitigation potential, as well as actual mitigation ---". I have estimated the above-ground stock of accessible woody biomass to be an estimated 544 Gt wood, 9272 Gt C – over 10000 EJ). Accounting for below-ground woody biomass (150 Gt C) and soil carbon below the trees (600 Gt C), the total stock of C is in the region of 1020 Gt C. Then of course there is a carbon store in inaccessible forest and in grassland and some in crops, plus the store in the soils beneath these land use types.	Unfortunately, no reference provided to properly evaluate and address comment. Appears to be similar to previous comments, so please see those replies (e.g., line 770 and 778).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8692	6	54	15			Section 6.4.1 is very good. You might want to add at the end that the implication of this section is that IAM modeling and scenario creation is for the purpose of achieving certain climate and social targets in the future, as knowledge of the earth/climate system increases. Because the mitigation trajectory actually followed by the world will be evolving based on new information, it makes no sense to talk about the probability as of today of any climate mitigation scenario occuring. And uncertainties in our current knowledge base will get reduced over time as we learn more, and as we take corrective action when the chosen mitigation trajectory gets off course. To me this should be the major theme of the entire chapter 6, and this material might best be put up front in the introduction to the chapter.	Agreed, the notion of sequential decision-making is an important theme. The author team will work on adding references to studies explicitly examining stochastic control and will incorporate the notion into the introduction section. In addition, a clearer discussion of how to interpret scenarios in the context of uncertainty about input parameters will be added to the introduction section of the chapter.
9897	6	54	33	54	34	"models or scenarios that assume the future availability of a negative emissions energy conversion technology" should be presented more in detail or at least a reference should be made. Otherwise I doubt how decision makers might perceive this statement. They might see this as a call for non-action.	The statement is an objective assessment of the results in published stabilization scenarios. It is an important characteristic of the ensemble of published scenarios that pathways with the possibility of negative emissions in the future can have higher emissions in the near-term while meeting the same target at the end of the century (the extent of overshoot notwithstanding). This finding is discussed further in the section above on stabilization pathways.
9067	6	54	14	58	7	6.4 Integrating long term and short term perspectives can be deleted due to limitations on the nos of pages	The structure of the chapter is being revised to best cover the material within the prescribed outline. However, we likely need to keep some or most of the material in this section. Every effort will be made to minimize overlaps and redundancies.
6917	6	54	16	54	20	Suggest to refer to the WGI AR5, Chapter 12 assessment for long-term climate change considerations.	We will include the reference.
10995	6	54	24	54	26	In this sentence, what does "the most relevant decisions" actually mean? It should be clearly stated.	"Most relevant" meaning those most important for analysis to inform. We will clarify the text.
6918	6	54	40	54	42	Refer to WGI AR5, Chapter 11 for an assessment of uncertainty in near-term forcings and of near term climate change.	We will include the reference (both here and above in Figure 6.3).
6281	6	55		55		Remove "ORNL" from the graphic and replace with "History" who compiled these data are not the important point to convey in this graphic.	Agreed.
15222	6	55				Figure 6.34, Range for Copenhagen Pledges in the graph needs to be clarified where the range is.	We will include a reference and corresponding numerical data for the Copenhagen Pledges in the graphic.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6105	6	55				In Figure 6.34, there is a red dot in 2050 showing G8 target. As far as I know, G8 always declared between 2007, at Heiligendamm and 2011, at Deauville on the need to reduce global emissions by 50% by 2050. But throughout its declarations, leaders never say from when. In 2007 at Heiligendamm, declaration stipulates "49. In setting a global goal for emissions reductions in the process we have agreed today involving all major emitters, we will consider seriously the decisions made by the European Union, Canada and Japan which include at least a halving of global emissions by 2050". At that time some countries thought the base year should be 1990 and others thought it as 2005. This was the reason why the base year remain unclear. I have checked Declaration in 2008 Toyako Summit, Japan, 2009 L'Aquilla Summit, Italy, 2010 Muskoka Summit, Canada, 2011 Deauville Summit, France and 2012 Camp David, USA. The wording is almost same as that in Toyako Summit in 2008 that "We seek to share with all Parties to the UNFCCC the vision of, and together with them to consider and adopt in the UNFCCC negotiations, the goal of achieving at least 50% reduction of global emissions by 2050 ---", except that in 2012 where no reference was made to 50% reduction. In this sense, G8 Target shown in the Figure 6.34 is quite unclear. However, I have found in page 57 line 7-8 the following expression, i.e. "target proposed by the G8 of a 50% reduction relative to 2000 ---". From the above, "G8 Target" should be replaced by "50% reduction from 2000".	We will clarify this data point with a reference and explicit defintion.
3152	6	55	10			This chapter has a few figures that could be iconic for the WG3 overall. They include: figs 6.5, 6.6, 6.7 and 6.34. As you trim the chapter pls try to keep those figures and work with TSU to make them clear. For example, add historical data to figure 6.7 to help put the pathways into context.	OK. The author team will work in improving the graphic in Figure 6.7 to see whether historical data can be incorporated.
7684	6	55	11			The first clause should already mention what the ranges mean, e.g. "… CO2 emission ranges in scenarios with Category 0 to Category 6 radiative forcing targets". The comment aplies also to Figure 6.35. Also change "AMPERE protocol" to "AMPERE project".	We will clarify the figure caption and description.
6282	6	56		56		Remove "ORNL" from the graphic and replace with "History" who compiled these data are not the important point to convey in this graphic.	Agreed.
9898	6	56	12	56	13	Deviating from the cost-minimizing near-term emissions profile does not necessarily increase global costs of meeting a long-term stabilization goal. Proper life cycle costing calculations can support decision making for the cost minimal solution in the long run.	Integrated assessment models take into account "life-cycle" costs already and can be used to calculate a stabilization pathway that minimizes these costs. By definition, deviating from this pathway will increase cost.
16723	6	56	12		15	Clarify please -- low cost options to reduce emissions to meet the 2 degree target are expiring -- we are going to be left only with much more expensive options if we continue on current course.	Right, the cost dimension is important here. We will work on revising the text here to bring this out.
8113	6	56	25	57	2	This paragraph might consider including: Rogelj, J., McCollum, D., O'Neill, B. & Riahi, K. Feasible 2020 emission windows for staying below 2°C during the twenty-first century. Nature Climate Change (in review, 2012).	We will include the reference.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9899	6	57	10			Please provide the references for the "published scenarios about option value". Otherwise questions like: which decision-makers are addressed, politicians on a global, national or local scale or managers?	This sentence should be reworded to avoid grammatical confusion. It is intended to say that broad conclusions about option value can be drawn from the literature of published scenarios. It is not referring to specific publications about option value.
11429	6	57	11	57	13	The statement in these lines that "there is some evidence that an emissions pathway through 2020 that follows the pledges in the Copenhagen Accord preserves the option of achieving a long-term target in the range of 450 CO2-e" should be qualified and explained with respect to its bases and its assumptions. A balancing statement or discussion should be provided that wuld also show what the other evidence might indicate in terms of the Copenhagen/Cancun pledges, particularly of developed countries, being insufficient and needing to be scaled up Without such an explanation or balancing statement, the current text could be taken by readers of IPCC AR5 as an implicit policy endorsement by the IPCC AR5 that developed countries' pledges under the Copenhagen Accord/Cancun Agreements are already sufficient and that they no longer need to show much greater ambition in terms of their mitigation targets for the period up to 2020. This could be taken by many, especially from developing countries, to mean that the burden for future mitigation efforts should therefore be on developing countries. If this becomes the case, then the scientific credibility and neutrality of the IPCC could become subject to challenge as it could be seen as having shifted from being a scientific body into becoming a policy recommending body with built in biases in favor of developed countries.	The statement is an objective assessment of the results in published stabilization scenarios. There are in fact several scenarios in which only the Copenhagen targets are enforced through 2020 but that in the long run can reduce emissions sufficiently to meet a 450 CO2-e target in 2100. The converse - that in other scenarios enforcing only Copenhagen through 2020 makes the achievement of 450 CO2-e by 2100 impossible - is difficult to deduce given the less than systematic reporting of infeasibilities. On the other hand, the cost dimension is important, and we will work on revising the text to bring out the result that doing "only Copenhagen" through 2020 raises the costs of 450 relative to an optimal path. More generally, the thrust of the comment seems to be about burden sharing and the allocation of mitigation responsibility among countries, whereas the conclusions being drawn in the current section concern only the implications of a global emissions level in the near-term for the abiliy to meet a global target in the long run. The current text very clearly avoids "endorsing" any particular policy, and certainly makes no evaluation (neither normative nor descriptive) of the de facto burden sharing implied by the Copenhagen pledges.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12310	6	57	20	57	23	The use of the term "institutions" might be too limiting since this isrelated to climate releated policy instruments in a broader sense.	Could not the capacity to implement a particular policy instrument be considered an "institution"? Perhaps "institutional capacity"? We will work on it.
11755	6	57	36	57	38	Even though ETS and carbon tax are examples, readers could misunderstand such institutions are better than others.Howard Geller and Jakin Nordqvist show the effectiveness of energy efficiency labeling, Japan's Top Runner Programme in their respective paper. [such as domestic and international emission trading.....with carbon pricing] should be deleted. 1.Howard Geller (2005):[The Experience with Energy Efficiency Policies and Programmes in IEA Countries : Learning from the Critics.IEA Information Paper], http://www02.abb.com/db/db0003/db002698.nsf/ca7e93ab03030d22c12571380039e8fc/0912873430b22467c12571da0032d460/$FILE/The+Experience+With+Energy+Efficiency+Policies+and+Programmes+in+IEA+Countries.pdf 2.Joakim Nordqvist (2006):[Evaluation of Japan's Top Runner Programme within the framework of the aid-ee project], http://www.aid-ee.org/documents/018TopRunner-Japan.PDF	It is our reading of the literature that market-based policies are in most cases the best mechanism for achieving deep emisisons cuts at minimal economic cost. It is also true that policies such as EE standards and labeling have been shown to be effective at overcoming information- and related externalities associated with consumer purchases. However, we do not see evidence that this type of policy can act as a substitute or equivalent alternative to a market-based emissions policy.
9578	6	57	36	57	37	Please, add following as good examples of no market mechnism; Howard Geller[1] showed the effectiveness of energy efficiency labeling as follows-In Europe, the average efficiency of new refrigerators and freezers was static or even declining prior to directives on energy efficiency labelling and standards. Thus the 27% decline in the average electricity use of new refrigerators and freezers sold in the EU between the early 1990s and 1999 was attributed to labelling and standards. Nordqvist[2] also evaluated Japan's Top Runner Programme to indicate that the Top Runner approach might contribute to about one sixth or more of the total Japanese savings ambition by 2010. [1]Howard Geller (2005) The Experience with Energy Efficiency Policies and Programmes in IEA Countries: Learning from the Critics. IEA Information Paper http://www02.abb.com/db/db0003/db002698.nsf/ca7e93ab03030d22c12571380039e8fc/0912873430b22467c12571da0032d460/$FILE/The+Experience+With+Energy+Efficiency+Policies+and+Programmes+in+IEA+Countries.pdf [2] Joakim Nordqvist (2006) Evaluation of Japan's Top Runner Programme within the framework of the aid-ee project http://www.aid-ee.org/documents/018TopRunner-Japan.PDF	It is our reading of the literature that market-based policies are in most cases the best mechanism for achieving deep emisisons cuts at minimal economic cost. It is also true that policies such as EE standards and labeling have been shown to be effective at overcoming information- and related externalities associated with consumer purchases. However, we do not see evidence that this type of policy can act as a substitute or equivalent alternative to a market-based emissions policy.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10651	6	57	36	57	38	Readers may misunderstand domestic and international emissions trading markets could only produce good devidents. But Howard Geller and Jakin Nordqvist argue the effectiveness of energy efficiency labeling, Japan's Top Runner Programme in their respective paper. [such as domestic and international emission trading.....with carbon pricing] should be deleted. 1.Howard Geller (2005):[The Experience with Energy Efficiency Policies and Programmes in IEA Countries : Learning from the Critics.IEA Information Paper], http://www02.abb.com/db/db0003/db002698.nsf/ca7e93ab03030d22c12571380039e8fc/0912873430b22467c12571da0032d460/$FILE/The+Experience+With+Energy+Efficiency+Policies+and+Programmes+in+IEA+Countries.pdf 2.Joakim Nordqvist (2006):[Evaluation of Japan's Top Runner Programme within the framework of the aid-ee project], http://www.aid-ee.org/documents/018TopRunner-Japan.PDF	It is our reading of the literature that market-based policies are in most cases the best mechanism for achieving deep emisisons cuts at minimal economic cost. It is also true that policies such as EE standards and labeling have been shown to be effective at overcoming information- and related externalities associated with consumer purchases. However, we do not see evidence that this type of policy can act as a substitute or equivalent alternative to a market-based emissions policy.
6499	6	57	36	57	39	This sentence should be eliminated. Because short-term mitigation efforts should not be limited to developing of domestic and international emissions trading market and carbon pricing.	It is our reading of the literature that market-based policies are in most cases the best mechanism for achieving deep emisisons cuts at minimal economic cost. It is also true that policies such as EE standards and labeling have been shown to be effective at overcoming information- and related externalities associated with consumer purchases. However, we do not see evidence that this type of policy can act as a substitute or equivalent alternative to a market-based emissions policy.
11428	6	57	4	57	13	The references to the Copenhagen targets or the Copenhagen Accord should be replaced with references to the Cancun targets or the Cancun Agreements. While the substantive content of these two instruments - particularly with respect to the emission reduction pledges or targets of various countries - were essentially the same, the legal status of these instruments in relation to the UNFCCC policy regime are not equal. The Copenhagen Accord and the targets pledged under it were not adopted by the UNFCCC Parties but were only noted, whereas the Cancun Agreements (decision 1/CP.16) were adopted by the Parties - thereby giving the latter a stronger and more durable normative policy standing under the UNFCCC policy regime.	The reference to the national pledges adopted under the UNFCCC will be clarified and standardized across the report.
12311	6	57	40	58	2	This paragraph could include some more about the implementation and deployment and the possible barriers related to implementation and deployment.	The author team will consider expanding the discussion to include barriers to implementation and deployment - presumably in the current context the comment refers to identifying and removing potential barriers as a benefit of early deployment?
16724	6	57	40		48	Highlight -- very important.	This is a key message of the section.
3297	6	57	16	58	7	This is good section. Keep it.	OK.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9900	6	58	17	58	27	During the life of a technology diverse barriers might emerge. This paragraph should be elaborated to raise awareness among decision-makers that the "complex process of interactions" has to be managed and can be managed.	We are not sure what precisely the reviewer means with "complex process of interactions", but we have made more explicit that barriers that might emerge must be managed appropriately if possible.
9896	6	58	42			the models don't "predict" this, this effect is built into the models as the prior sentence makes clear.	Yes, we agree that the choice of the word 'predict' is unfortunate here.
8693	6	58	42			the models don't "predict" this, this effect is built into the models as the prior sentence makes clear.	See the previous comment and our reply, which somehow is duplicated.
16725	6	58	7			Suggest insert: However, market participants will not invest in development or deployment of large, low emitting technologies if they lack confidence in the political commitment to the carbon pricing system. Investment decisions are made looking at the long term price outlook, while operating decisions are made based on the current, or spot price of emissions.	This is an important point. The author team will work on ways to incorporate this point into the discussion.
6283	6	58	9	58	38	There are a few statements in this section that while I might agree with personaly, I am not sure they belong in this report or if there is sufficient literature that can be cited to substantiate the point. "The likelihood of a unified global policy for greenhouse gas mitigation is low for the near future."	We same to agree here, but recognise the reviewer has a point; we have reformulated this and the subsequent sentence.
9068	6	58	8	63	48	6.5 Integrating technological and societal change can be deleted due to limitations on the nos of pages	The authors have been instructed to dwell on this subject at this place in the report and chapter, so we cannot delete it. But efforts have been made to write this section more concisely and to the point.
6284	6	58	10	60	33	The vast majority of this nearly three pages of text is about how integrated assessment models deal with technological change. That is it is process oriented. Given the need to reduce the length of this chapter significantly, it would seem that much of this material could be cut back, the relevant literature could be cited and then you could get to the outcome / what the result is; which is that technology is important because it reduces cost significantly lines 7-14 on page 60.	Yes, our discussions seem to go into the direction of cutting this section back to some extent, even while it is hard to cite specific references as the literature on this subject is so large. Also, during the revising of the FOD and SOD we will have to make sure that these elements are appropriately addressed in either chapters 3 or 6, as they concern an important topic, while simultaneously precluding any duplication between these two chapters.
3298	6	58	8			This is good section, especially page 58, line 39 to page 59, line 3.	Noted. We will account for this while nevertheless attempting to somewhat reduce the length of this section.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4209	6	58				The term "risk" should be more clearly defined. There are some risk categories in the deployment of new technologies. For instrance, when the certaintechnology is implement only in the small portion, then cost is high whle the outcome is negligible small (or negative, in some cases). In case of carbon tax, partial implementation will results a large distortion and thus none will consider it seriously afterwards. This is an example of bad taxtics. Second, there is a counter-risk when an option is widely implemented. This is discussed well. Third, there can be a risk when a large implementation is failed. This is a business risk but can cause additional societal risk such as finantial crisis.	The term risk will be clearly defined in the AR5 glossary, and Section 6.7 will adhere to this. The introduction of the section will focus on clarifying the type of risk addressed in the section (the risk of mitigation failure) and distinguish it from risk trade-offs (Chapter 6.6) and risk analysis (Chapter 2)
15223	6	59				Figure 6.36 needs to be clarified.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
4205	6	59				hard to read!	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
5872	6	59				Figure is not legible, please rework.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
16727	6	59	11		20	Do we need the references to the models, or here are the main points and then list the citations.	There was an editing problem in the creation of the references for the FOD. We will make sure this doesn't happen again in the SOD
10794	6	59	31			Figure is garbled and confusing. Please redesign	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
13174	6	59	32	59	38	For the models implementing e.g. learning curves, it would also be useful to indicate whether they assume perfect foresight with intertemporal optimization. This is likely to have a big impact on model decisions, as the model knows the "winning technologies" beforehand (i.e. no uncertainty) and knows also how much having these technologies is worth in the future (i.e. there is no uncertainty about anything else, such as climate target, either).	Noted. We now briefly refer to this.
14456	6	59	32			This graph is illegible.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
4206	6	59	4	59	20	The effects of learning curve is that the increase of cummulative production induces cost reduction and promotes further implementation. This means, from another side of view, that small implementation in the early stage weakens the penetration. Thus, technology susbtitution is delayed or never implemented (lock-in effect). It seems to me this inverse efffect should be also touched upon here.	True. We have now included a sentence along these lines in the new version of this section.
16726	6	59	8			Replace "fossil fuels" with "high-emitting technologies". Fossil fuels can be useful with the correct technologies.	Yes , that is true. We have changed this.
4768	6	6				Are categories represent the different range of "radiative forcing in 2100 (W/m²)" ?	Noted. Categories will be clearer in the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5854	6	6				Please indicate what is given in the brackets (ranges, standard deviation, …).	Noted.
8055	6	6				For me the information in Table 6.2 is at least as relevant as in Table 6.3 (which became Table ES.1). Why not include all data from the two tables in ES.1 ?	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. In addition, it is not clear whether any tables will remain in the ES in the SOD.
8614	6	6	10			The top row of this table should be labelled "Year of Peak Emissions" and just "Peak Emissions". Also, the last three columns must be labeled as percentages.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. In addition, it is not clear whether any tables will remain in the ES in the SOD.
12305	6	6	12	6	16	This part focus on technology strategy. It is also important to see this in connection with climate change policies in a broader context, not only in relation to technology. This also involves how to handle risk sharing in a situation where there is a need for a shift in technology. There is more risk and higher cost involed for the early movers. Hence, this might create a need for other policies, especially in the transformation phase. This should also be reflected in the body of the text, for instance section 6.7 and/or section 6.3.5.	Noted.
13123	6	6	13	6	15	The wording is again too brave. Nobody knows what WILL happen in the scope of an almost 100 year long transition. These are model outcomes, reflecting very specfic sets of assumptions and the conclusions should be framed with that in mind (i.e. do not reformulate model outcomes as forecasts, but keep it clear that the statement about "predictions or forecasts" (page 15 line 6) is still valid)	Rejected. All the evidence that has been reviewed in this chapter indicates that to meet ambitious concentration goals will require a very different energy system than the one of today.
2255	6	6	15	23	19	Again, this mystrious preoccupation with EMISSIONS when the supposed theoretical influence is atmospheric CONCENTRATIONS. There is no scientificall established relationshio between the two	Rejected. This is an issue for WGI.
4769	6	6	17	6	20	I fully agree however it is difficult to put an economical value to all those elements.	Noted.
9827	6	6	17-20			As already mentionned, this perspective does not allow for the possibility of macro-economic benefits, which would make it much easier for policy makers to choose a transformation pathway over a reference scenario. Moreover the time perspective should be integrated: as the financial crisis demonstrates the shortterm macroeconomic costs are considered in decision making: they can be calculated quite easily based on predictions and they are relevant to politician as arguments in election campaigns. But longterm macroeconomic costs and benefits are often neglected.	Noted. The issue of negative costs will receive mention in the chapter, although it is unlikeley it will make it to the ES given space constraints.
8615	6	6	17-20			This sentence perfectly illustrates my point above. It does not allow for the possibility of macro-economic benefits, which would make it much easier for policy makers to choose a transformation pathway over a reference scenario.	Noted. The issue of negative costs will receive mention in the chapter, although it is unlikeley it will make it to the ES given space constraints.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2181	6	6	2	6	4	Expressing the stabilisation level as a radiative forcing is fine, but will not be very helpful to most readers. It should be possible to presnt this as transient or equilibrium temperature change with dialogue with WGI.	Rejected. At present, given the uncertainties in the relationship between concentrations and temperature, a clear methodology has not been articulated to express concentration pathways in terms of temperature. At the same time, the chapter will be modified to include a section that attempts to explain the link between temperatures and concentrations.
13124	6	6	25	6	27	The range of model outcomes (which I assume this refers to) does not automatically provide a mapping of real life uncertainties. Also, as mitigation costs are a function of the baseline AND the cost range understandably increases the further away one is from the baseline, couldn't one alternatively interpret this range as reflecting the uncertainties of the baselines? Finally, one would expect the energy (and other) systems of 2100 to be quite different from those of today, no matter what is assumed for the mitigation target. Rephrase.	Noted. These are all good points.
8616	6	6	25-27			This sentence is correct - but it should say "net cost or benefit estimates" not "cost estimates". The reader needs to be clear that costs or benefits are measured relative to a baseline scenario's costs. The point is that the uncertainty in net costs goes in both the positive and negative directions depending on the values of all the input assumptions. This is another reason, supporting my point above, why the critical role of the variations in input assumptions between models should be explained.	Noted. The issue of negative costs will receive mention in the chapter, although it is unlikeley it will make it to the ES given space constraints.
14392	6	6	9			Define RC	Accepted. We will define the RCPs in the chapter.
6095	6	6	9			It will be better for reader friendliness purpose to add concentration level and, if possible, temperature increase expressed in terms of probability. Alternative simplified idea is to refer to Table 6.2.	Noted. At present, given the uncertainties in the relationship between concentrations and temperature, a clear methodology has not been articulated to express concentration pathways in terms of temperature. At the same time, the chapter will be modified to include a section that attempts to explain the link between temperatures and concentrations. More generally, the ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD, so this table may not remain.
11250	6	6				In the Executive Summary it is said that “dramatic changes” are required and that “dramatic expension” of low-carbon sources has to be included. But there is no reference to the chapter. Where can these "dramatic” changes be seen?	Noted. The changes in the energy system can be found in the section on energy system transitions.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
10980	6	6	25	6	27	Why "such estimates must be based on characterizations of energy and other systems that are very different from those of today"? Does it mean the composition of future energy supply will widely and drastically change in comparison with that of current one?	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. Regardless, a major result from the literature in this chapter is that stabilization will require a very different energy system than that of today.
15224	6	60				Figure 6.37 needs to be clarified.	There was an editing problem in the creation of the figures for the FOD. We will make sure this doesn't happen again in the SOD
6407	6	60	29	62	30	I know this is the FOD, but filling in the references here would be helpful.	There was an editing problem in the creation of the references for the FOD. We will make sure this doesn't happen again in the SOD
13175	6	60	8	60	8	Should the numbers 1-6 be replaced with references?	There was an editing problem in the creation of the references for the FOD. We will make sure this doesn't happen again in the SOD
17809	6	61				please refer to the document, UN 2012. From transition to transformation. Sustainable and inclusive development in Europe	Noted.
9901	6	61	0			It would be very good if the case can be made here even more strongly that increasing the level of R&D expenditures quite substantially is still bound to be highly cost effective by providing some rough estimates of how big the long run benefits might be from improved low carbon technologies.	We will expand this part with newer studies, if they become available.
8694	6	61	0			It would be very good if the case can be made here even more strongly that increasing the level of R&D expenditures quite substantially is still bound to be highly cost effective by providing some rough estimates of how big the long run benefits might be from improved low carbon technologies.	Will expand with newer studies, if available
3299	6	61	1			The table is fine, but it needs more explanation in the title paragraph and/or the text about it.	Yes, we agree. We particularly also emphasize the uncertainties associated with these figures.
3149	6	61	1			This chapter has a few figures that could be iconic for the WG3 overall. They include: figs 6.5, 6.6, 6.7 and 6.34. As you trim the chapter pls try to keep those figures and work with TSU to make them clear. For example, add historical data to figure 6.7 to help put the pathways into context.	ok
16728	6	61	10			point made that policy must be credible -- this is very important for investors in big, long lived assets. Can you explain a bit more for people?	We could, but given the length limitations we may best refer that task to the policy chapters 13-15.
6106	6	61	15	61	16	There is a sentence that "alternatively, carbon taxes greater than the Pigouvian level are recommended when one accounts for market imperfections in the knowledge sector (REFERENCE)". Pigouvian tax is a tax that materialize not only cost effectiveness but also economic efficiency (relying upon cost benefit analysis). Whereas in page 26, this chapter says that CBA is not approapriate for the purpose of discussiing stabilization pathways because CBA leads to increasing concentration. The expression here is inconsistent with the above expression. Also reference is absolutely needed.	Pigouvian taxation is independent of CBA analysis. The point here is to show that multiple policies can be welfare improving when there are multiple externalities

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14039	6	61	31	63	48	In this part it would be appropriate to refer to wider interpretations of transformation and how this relates to sustainability pathways. Literature to consider: National Research Council. 2011. Climate Stabilization Targets: Emissions, Concentrations, and Impacts over Decades to Millennia. Washington, DC: National Academies Press. WGBU. 2011. World in Transition: A Social Contract for Sustainability. Summary for Policy-Makers, Berlin:Wickson, F. A.L. et al 2006. Transdisc. research: characteristics, quandaries and quality. Futures 38: 1046-1059; Raskin, P. et al. 2002. Great Transition: The Promise and Lure of the Times Ahead. SEI,Tellus Institute; Brown, L. 2010. PLAN B 4.0. New York: W.W. Norton and Company; Leichenko, R. and K. O'Brien, 2008. Environmental change and globalization: Double exposures. Oxford Press. ; Leiserowitz, A. A., R. Kates, and T. M. Parris. 2006. Sustainability values, attitudes, and behaviors: A review of multinational and global trends. Annu Rev. Environ. Resour. 31: 413-44; Pelling, M. 2010. Adaptation to Climate Change: From Resilience to Transformation. London: Routledge; O'Brien, K. 2011. Global Environmental Change (2): From Adaptation to Deliberate Transformation. Progress in Human Geography. Published Online 10 November 2011; Westley, F., Olsson, P. Folke C. et al. 2011. Tipping Towards Sustainability: Emerging Pathways of Transformation. 3rd Nobel Laureate Symposium on Sustainability, Stockholm.	These are valuable suggestions, as indeed we do not only want to refer to integrated assessment modeling work on transformation pathways, but bring forward wider interpreattions of these pathways.
13176	6	61	7	61	8	Give references.	There was an editing problem in the creation of the references for the FOD. We will make sure this doesn't happen again in the SOD
10996	6	61	18	61	20	What does the term of "behavioral anomalies" mean concretely? It should be clearly stated.	Yes, we agree: we clarified this.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9902	6	62	0			An analysis of 104 empirical studies of innovation to change showed the following barriers, that could refine and structure the discussion of barriers: Issues of resourcing (76%), for instance, “not enough resources” (Post and Altman 1994), “lack of adequate resources such as time and staff” (Adams and McNicholas 2007), limited or no budgeting (e.g. Harris 2000 and Anumba et al. 2006), access to capital and lack of time (Rohdin and Thollander 2006). Issues of capabilities (75%), for instance, “low technology literacy” (Stewart, Mohamed and Marosszeky 2004), “ill-equipped in terms of training and expertise” (Whitaker 1987), “employees are not trained” (Tamimi and Sebastianelli 1998), “lack of understanding” (Waldron 2005), “lack of technical skills” (Rohdin and Thollander 2006), “lack of skill, knowledge and expertise” (Kirkland and Thompson 1999), etc. Issues of communication (64%), for instance, “communication barriers” (Heide, Grønhaug and Johannessen 2002), “communication overload and distortion” (Allen 2002), “lack of communication within the team” (Attaran and Nguyen 1999), “lack of communication among those sharing responsibility for different aspects” (Kunda and Brooks 2000), “poor communication practices that damaged employee commitment to projects” (Jacobs et al. 2006), “tension among departments arising from the incompatibility of actual or desired responses” (Aggarwal 2003), etc. Issues of organizational structure (62%), for instance, bureaucracy (e.g. Molinsky 1999; Borins 2000; Abdul-Hadi, Al-Sudairi and Alqahtani 2005), “salary structure” (Al-Qirim 2007), “complexity, centralization, and formalization”(e.g. Allen 2002), “rigid organizational boundaries” (Butler 2006), “departmental fortresses” (Cicmil 1999), and organizational structure (e.g. Scarbrough and Lannon 1988; McGaughey and Snyde 1994; Yauch and Steudel 2002). Abdul-Hadi, N., Al-Sudairi, A. und Alqahtani, S. (2005): Prioritizing barriers to successful business process re-engineering (BPR) efforts in Saudi Arabian construction industry, In: Construction Management \& Economics, Vol. 23, Nr. 3, S. 305-315. Adams, C.A. und McNicholas, P. (2007): Making a difference: Sustainability reporting, accountability and organisational change, In: Accounting, Auditing and Accountability Journal, Vol. 20, Nr. 3, S. 382-402. Aggarwal, N. (2003): Organizational Barriers to Market Orientation, In: Journal of Management Research, Vol. 3, Nr. 2, S. 87-97. Allen, R.Y.W. (2002): Assessing the impediments to organizational change: A view of community policing, In: Journal of Criminal Justic, Vol. 30, Nr. 6, S. 511-517. Al-Qirim, N. (2007): The adoption and diffusion of E-commerce in developing countries: The case of an NGO in Jordan, In: Information Technology for Development, Vol. 13, Nr. 2, S. 107-131. Anumba, C.E.H., et al. (2006): Understanding structural and cultural impediments to ICT system integration: A GIS-based case study, In: Engineering Construction & Architectural Management, Vol. 13, Nr. 6, S. 616-633. Attaran, M. und Nguyen, T.T. (1999): Design and implementation of self-directed process teams, In: Management Decision, Vol. 37, Nr. 7, S. 553-561. Borins, S. (2000): What Border? Public Management Innovation in the United States and Canada, In: Journal of Policy Analysis and Management, Vol. 19, Nr. 1, S. 46-74. Butler, J.C. (2006): Ten Lessons Learned: Data Warehouse Development Project, California Department of Fish and Game, In: CrossTalk: The Journal of Defense Software Engineering, Vol. 19, Nr. 10, S. 16-20	Thanks for this long list of suggestions, which we have considered.
16729	6	62	34			Insert at the beginning of section "the difference between engineering potential vs. market potential"	Rejected. The list is about drivers of the energy efficiency gap. What you mention here are two different metrics.
16730	6	62	38			Also due to capital budget constraints and decision makers preferring non-energy related investment options if they have a higher relative return.	Noted.
6408	6	62	9	62	9	"all the more so after Fukushima" is conjecture and should be reworded.	Noted. This section was completely rewritten in the new draft and does not contain this matter anymore.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
5925	6	62	9			Suitable references to nuclear policies after Fukushima are: 1. Globally: P. Joskow, J. E. Parsons, The Future of Nuclear Power After Fukushima, Econ Ener Env Pol 1(2) (2012) 99-113, and 2. Concerning EU countries: Syri S., Kurki-Suonio T., Satka V., Cross S., Nuclear power at the crossroads of liberalised electricity markets and CO2 mitigation - case Finland. Energy Strategy Reviews (accepted with minor rev.)	Noted. This section was completely rewritten in the new draft and does not contain this matter anymore.
3300	6	62	34	62	38	This is an important paragraph. It needs more explanation to 'bring it up to' other paragraphs' length and depth.	we will improve the link between this paragraph and the rest of the section
10954	6	62	44	62	44	Fischer et al. (2011) missing in reference list.	Rejected. The reference actually *is* in the reference list ("Fischer & al 2011").
18640	6	63				Page 63: The challenge is to avoid self-reinforcing loops between technical choices, life-styles and institutions which result in a carbon intensive lock-in.	we will give one example of this loop like the link between urban sprawl, the structure of transportation modes and the demand for mobility
17474	6	63	14	63	15	Meaning not clear to me	difficult for us to see what you think unclear in this page. We will anyway clarify and complement this section
9903	6	63	18			Changes in consumption patterns are mentioned here, but I think much more attention ought to be given to the basic issue of economic growth in this sub-section. As you know, many advocates of "no growth", "low growth", etc. have become much more active over the last several years, and the world economic crisis has fed into concerns about how much economic growth is sustainable, and compatible with climate change mitigation. Yet, I don't believe that the IAM literature has many climate change mitigation scenarios that reflect these debates by doing sensitivity analyses using much lower economic growth rates for certain regions of the world, especially the OECD countries, than the growth rates used in the base case scenarios. These types of sensitivity cases should have been run by more IAM modeling groups by now, but even if the literature on this issue is skimpy, the issue should be discussed in this section of the report. Preferably, this section should be moved up front in the chapter to where all input assumptions are presented together, as I have previously advocated. It is important to discuss low economic growth scenarios because economic growth is one of many key policy levers that could be relied on to reduce greenhouse gas emissions, if needed.	we understand the point; and we will examine seriously your proposal. However, it will be more easy to do so, for reasons of time constraints; in the 2nd order draft. We retaing anyway the point that questions about changes in consumption patterns relate to the even more fundamental question of economic growth in matured economies
8695	6	63	18			Changes in consumption patterns are mentioned here, but I think much more attention ought to be given to the basic issue of economic growth in this sub-section. As you know, many advocates of "no growth", "low growth", etc. have become much more active over the last several years, and the world economic crisis has fed into concerns about how much economic growth is sustainable, and compatible with climate change mitigation. Yet, I don't believe that the IAM literature has many climate change mitigation scenarios that reflect these debates by doing sensitivity analyses using much lower economic growth rates for certain regions of the world, especially the OECD countries, than the growth rates used in the base case scenarios. These types of sensitivity cases should have been run by more IAM modeling groups by now, but even if the literature on this issue is skimpy, the issue should be discussed in this section of the report. Preferably, this section should be moved up front in the chapter to where all input assumptions are presented together, as I have previously advocated. It is important to discuss low economic growth scenarios because economic growth is one of many key policy levers that could be relied on to reduce greenhouse gas emissions, if needed.	same question, same response

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9904	6	63	26	63	28	Life cycle costing should be explicitly mentionned here as an instrument that can assist decision makers in assessing tradeoffs, such as between commuting and housing.	we are not sure that the Life Cycle Costing methods are mature enough to assist decisions makers in assessing the trade offs between commuting and housing. We will see whether such analyis exist. If not we will pinpoint the necessity to develop assessement methodologies amongst which life cycle costing and urban scenarios
6694	6	63	3	63	7	Good text. A big issue of carbon tax is that a family budget is damaged by it.	thanks
9500	6	63	3	63	7	good issue - It is obviously mentioned that a carbon tax has a bad influence for consumers	thanks
6409	6	63	30	63	31	I don't understand what "is a way of controlling the induction of automobile dependant transportation patterns" means	we will explain more clearly that investment in infrastructure combined with appropriate energy and real estate pricing determine the amount of mobility needs which can be fulfilled only by private cars
17420	6	63	35			"Another critical sector here is agriculture and food production." It is not particularly helpful here to simply refer to these sectors without some explanation of how and why they are critical.	good point. We will develop and give some pieces of littérature
16733	6	63	38		44	This presumes the market does not work to balance the utility of these activities as compared to the utility of cutting emissions in response to the carbon price -- in free economies, consumers act to maximize utility, not minimize costs. Emissions associated with some of the points you list may be very valuable to consumers (they have high willingness to pay). Example: Consumers may be willing to pay a lot for a flight for a vacation. The flight will become more costly with a carbon price -- yet consumers still want a vacation. If they take vacation with flight does this mean the policy failed? If the cap has integrity, the needed reductions are happening in activities with lower associated utility or very low cost reductions.	I am not sure I understand how you point is connected with the message of this para. I will try and be more explicit. The point is that, a) in a market economy with market (energy, real estate, land, labor) and institutional failures (including fiscal systems), a cap on emissions will entail welfare losses (as you say, activities with a lower utility or more costly) b) correction of these failures will likely not be made for reasons of climate policy only and will respond to other public objectives
6410	6	63	47			"unlock(ed)" overnight is colloquial and could be written "cannot be easily undone" or something more focused professional-sounding.	OK will be corrected
16731	6	63	7			These cost impacts can be mitigated via rebates or allocations of allowances to make price changes more gradual, giving consumers time to adjust.	OK we will suggest that.
16732	6	63	7		15	Good.	thanks

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16734	6	64				Is Sustainable Development defined well enough to be meaningful in the context of climate policy? Is there a discipline around this so that terms are defined or understood? Does it ask too much of a climate policy to also need objectives of Sustainable Development (however it is defined)?	Noted. The IPCC plenary saw the relevance to discuss mitigation in the context of broader sustainable development and for that reason dedicated an entire chapter (Ch.4) to this and requested it to be reflected in the respective chapters.
15225	6	64				section 6.6 seems to repeat the contents in chapter4: Sustainable Development. Please revise.	Noted. Chapter 4 is meant to provide framing (conceptual) whereas here the focus is on the applied side. We will ensure that sections/chapters build on each other and overlaps are avoided.
16735	6	64	17		43	From an economic perspective, this does NOT mean these policies lower the cost to reduce CO2 (CO2 price is not good proxy for economic cost). Rather, they force actions that would not happen unless the CO2 price was very high, meaning the $/ton cost of the action is very high. These policies hide the true costs of lowering carbon from the carbon market -- they do not result in additional emission reductions, only in the reduction of higher cost instead of relatively lower cost reductions. These "co-benefits" come at a very high, but hidden, CO2 price.	Noted. The statement here is about the level of carbon tax and not about aggregated economic or societal costs.
13177	6	64	21	64	25	This should be cross referenced with what is being said on page 36, lines 7-17 (suggesting that cap-and-trade mitigation is more expensive if it's combined with instruments targeting subsystems) and on page 30, lines 9-13 (emission price does not reflect full costs of mitigation, if additional policies affecting emissions are in place).	Noted. This section has been significantly restructured with the new draft.
9905	6	64	3			What is meant by "sustainable development" in this report should be described in the introductory section 6.6.1. Which time scale is considered and which dimensions of decision making?	Accepted. This section was revised, references to Ch.4 added.
8696	6	64	3			What is meant by "sustainable development" in this report should be described in the introductory section 6.6.1.	This comment is a duplicate of comment no 9905, please see for answer there.
3378	6	64	34			Another reference for co-benefits of urban transport climate change mitigation measures is: F. Creutzig, D. He (2009) Climate change mitigation and co-benefits of feasible transport demand policies in Beijing. Transportation Research D 14: 120-131. The "co-benefits" are of an order of magnitude larger than climate benefits.	Accepted. Congestion, air pollution, accidents and noise are now covered in Table 6.5 which is i.a. sourced from Section 8.7 which in turn contains a reference to the paper mentioned by you.
4207	6	64				This section is very important especially for the decision makers in de elopping coutries. More concrete case studies, reports and studies in development economics should be refered. Currently model simulations and some case studies are mentioned.	Accepted. We have now attempted this by introducing Table 6.5.
11430	6	64		64		This section could be improved through the incorporation of specific references to various provisions of the UNFCCC that reflect the linkage between the achievement of sustainable development and effective climate change actions - e-g- UNFCCC Arts. 2, 3.4, 4.7.	Noted.
17922	6	64	28	64	43	The literature and details covered in this paragraph are very interesting, but might or should be covered in the respective sector chapters. In my eyes, the role of chapter 6 would rather be to provide the link between the framing and sectoral discussions of SD and co-benefits/co-costs with the tranformation pathway literature and provide an overview of methodological challenges.	Accepted. We have now attempted this by introducing Table 6.5.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
17349	6	64	30	64	34	"Travel demand and choice of travel mode depend on land use planning interventions" there is no direct "dependency" or "causality" between travel demand, modal choice and physical spatial attributes of cities. The word dependency can here be rather misleading. The major part of the transport literature supports the proposition that travel is a derived demand, and both modal choice and phycical movement are more dependend on income, preferences, cost of transport, housing markets and demographic characteristics than a result of land use planning per se. The choice of words is important as well as the use of the most current literature. Suggest to use more recent literature here than (Cervero and Kockelman 1997), the latest appears to be with (Ewin and Cervero, 2010) which is OK. Problem seem to be that the academic literature will be strong on elasticities but scenarios cannot be built using them.	Noted. This section has been significantly restructured with the new draft.
17923	6	64	45	65	27	Please explain what is meant by 'Baseline Sustainable Development Policies and Actions' or paraphrase. These paragraphs seem to describe important caveats to conventional ways of accounting for costs of alternative baseline scenarios which are used throughout the chapter 6 which are partly based on the metrics discussion of chapter 3. At the same time, too little literature is provided (apart from the ecosystme service context) to substantiate the claims made. The part on different baselines in different regions is very promising but would need to be expanded (possibly by referring to the following paper: Steckel, Jan, Robert J. Brecha, Jessica Strefler, Michael Jakob und Gunnar Luderer (in review): Development without energy? Assessing future scenarios of energy consumption in developing countries. Working Paper. Submitted to Ecological economics (http://www.pik-potsdam.de/members/steckel/publications/development_energy_new)	Noted. This section has been significantly restructured with the new draft.
13178	6	65	1	65	2	This statement needs to be made more clearly. Is it meant to say that a given, non-minimized cost can be achieved with a number of technical systems? Or, in case least cost systems are discussed, should "identical" be preceeded with the word "nearly"? And why does this all depend on whether endogenous technical change is allowed?	Noted. This section has been significantly restructured with the new draft.
16028	6	65	14	65	21	Inapprehensible	Noted. This section has been significantly restructured with the new draft.
16736	6	65	21			Is it established that "leapfrogging" is indeed possible in any but the smallest state/region? Has this been well established in the literature?	Noted. This section has been significantly restructured with the new draft.
16737	6	65	27			add to last sentence: "with the understanding that within the economic context, forcing higher cost reductions via policy measures is a more costly and less efficient approach than allowing a CO2 price to shape consumption and investment choices which allows utility maximization within the society."	Noted. This section has been significantly restructured with the new draft.
2184	6	65	28	67	36	The key conclusions of this section need to be brought forward into the SPM. The conclusion that sustainability pathways have lower costs, additional co-benefits across sectors, improved health and wellbeing, greater equity, improved security, etc is probably the most important conclusion of the chapter, or even the entire WGIII report.	Noted. In the new draft co-benefit issues indeed have become part of the chapter ES and summary documents.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
11431	6	65	29	65	30	The reference to the "sustainable development and green economy paradigms" should be reformulated in order to reflect the latest multilateral consensus coming out from the Rio+20 summit on the relationship between sustainable development and the green economy - i.e. of green economy in the context of sustainable development and poverty eradication as one of the important tools available for achieving sustainable development and that it could provide options for policy making but should not be a rigid set of rules. The Rio+20 outcome document provides a lot of multilaterally agreed policy statements regarding how green economy is not considered as a paradigm separate from that of sustainable development - e.g. paragraphs 56-74, Rio+20 Outcome Document (see http://www.uncsd2012.org/content/documents/727The%20Future%20We%20Want%2019%20June%201230pm.pdf)	Noted. This section has been significantly restructured with the new draft.
16738	6	65	36			Suggest add sentence: "Working outside of linked, CO2 markets, it is difficult to imagine how transfers of the scale needed would be funded."	Noted. This section has been significantly restructured with the new draft.
13181	6	65	40	65	45	These seem like a rather courageous claims and presumably depend completely on how some of these benefits have been monetized. More references supporting these are needed, in any case, and I would even then suggest a more careful formulation, due to the difficulty of comparing costs against benefits that are non-trivial to monetize.	Noted. This section has been significantly restructured with the new draft.
13180	6	65	41	65	41	Reference is not included in the bibliography (or, alternatively, the given publication year is wrong_	Accepted. This section has been significantly restructured with the new draft, in this process this has been corrected.
9988	6	65	42	65	45	Low carbon technologies should include "heat pump technology" because heat pump has huge potential to reduce GHG emission, as described in (IEA, 2011, page16). This literature is listed in the No51 line of this table.	Noted. This section has been significantly restructured with the new draft.
16739	6	65	45			Suggest add after "etc." the following: "the benefits of which may be difficult to quantify."	Noted. This section has been significantly restructured with the new draft.
13179	6	65	7	65	11	There are some incomplete sentences on these lines.	Noted. This section has been significantly restructured with the new draft.
17924	6	65	32	65	36	Please provide a reference to substantiate the results in these sentences.	Noted. This section has been significantly restructured with the new draft.
10997	6	65	42	65	45	Why will higher upfront costs of low carbon technologies "be more than balanced by gains from fuel conservation, enhanced energy security, improved air quality etc."? I should be supported logically. Reference: IEA, Energy Technology Perspectives 2012 Pathways Clean Energy System, http://www.iea.org/Textbase/npsum/ETP2012SUM.pdf	Noted. This section has been significantly restructured with the new draft.
6541	6	65	44		45	Replace "additional costs are more than balanced" with e.g. "additional costs can be balanced", as the description is not always true and it is ease to find the opposite.	Noted. This section has been significantly restructured with the new draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16740	6	66	29		44	This is not supported by economic literature -- it reads as a sustainability advocacy piece. Suggest it be rewritten with more attention to economic literature looking at total economic costs rather than simply the modeled CO2 prices. I do not disagree with the point that it is less costly for developing countries as compared to already developed countries to become low emitting economies. This is supported by research -- this should be the focus without folding in the discussion of sustainability which touches on many other aspects of development besides climate.	Rejected/Accepted. The author team has been tasked to contextualize with sustainable development, for this reason this continues to be covered in the new draft. The new draft, though, indeed tries to further substantiate the findings as requested by you.
11432	6	66	29	66	29	Same comment as with page 65, lines 29-30, with respect to the relationship between sustainable development and green economy.	Noted. This section has been significantly restructured with the new draft.
13182	6	66	9	66	9	Unclear what/where this "table 1" is. First table in section 6.5 shows R&D needs.	Accepted. This reference has been corrected when restructuring this section.
16352	6	66	20			I have the impression that section 6.6 needs substantial revision and development. While section 6.3 provide costs, this section does not seem to link costs of stabilisation to sustainable development. The role of subsection 6.6.3 seems unclear, as it apparently focuses on a specific interpretation of the term 'low carbon society' (LCS) which is much narrower than the title would suggest (in particular, the last paragraph suggests that LCS is not viewed as a general concept but as a new and very specific framework).	Noted. This section has been significantly restructured with the new draft.
9299	6	66	1	66	3	Please add the following example and reference. ------(Shukla, Garg, and Dhar 2009). The regional cooperation between local government and cement industry generated co-profits to treat municipal wastes in cement kiln (MORIMOTO, NGUYEN, CHIHARA, HONDA and YAMAMOTO; Vol.2 No.4 2006, Journal of Life Cycle Assessment, Japan "Proposals for Classification and an Environmental Impact Evaluation Method for Eco-Services: Case study of Municipal Waste Treatment in Cement Production")	Noted. This section has been significantly restructured with the new draft.
13183	6	67	16	67	16	Unclear what/where this figure 1" is. First figure in section 6.5 shows technology specific cost trajectories.	Accepted. This reference has been corrected when restructuring this section.
13184	6	67	22	67	22	6.2.3. is interpreation of model infeasibility. Probably 6.3.3. was meant?	Accepted. This reference has been corrected when restructuring this section.
16029	6	67	25			include: reduction of artificial fertilizer	Noted. This section has been significantly restructured with the new draft.
16742	6	67	27		36	Suggest delete this paragraph. How is this supported? What does it mean? I can imagine poorly designed systems that do not include carbon from land use changes causing problems, but this can be solved via better market design. What else is this referring to?	Noted. This section has been significantly restructured with the new draft.
8698	6	67	27			This last paragraph does not make much sense, and is not supported by references to research. I would either leave it out completely or re-write it to state more defensible positions supported by research.	Noted. This section has been significantly restructured with the new draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16350	6	67	28	67	30	The sentence is not clear: what is the meaning of "increasing climate consequences" (following mitigation)? In addition, It is frustrating to make such a general and theoretical statement -- that spending efforts in an area (mitigation) might reduce efforts in other areas (adaptation and 'development') without actually summarising studies that assess this type of risk and conclude whether it is possible to avoid it or not. There should be at least a link to the appropriate sections of the report.	Noted. This section has been significantly restructured with the new draft.
11433	6	67	35	67	36	The statement that "sustainable development is an essential framework to align mitigation and adaptation policies and actions" could be further strengthened by linking it to the various provisions of the UNFCCC that shows such a relationship - e.g. UNFCCC Arts. 2, 3.4, 4.7	Noted. This section has been significantly restructured with the new draft.
16741	6	67	8		18	Should compare the costs of non-market based policies in terms of the amount of emission reductions achieved to the CO2 price policies which drive the same amount of emission reductions. You will undoubtedly find that the latter is much less costly. Yes, RD&D with state support can create large value for a society, but large scale deployment to meet other policy positions then requires non-market actors to decide how much of what produced by whom -- the problem of picking winners and losers. This socializes broader economic risks, removing it from the private sector.	Noted. This section has been significantly restructured with the new draft.
17926	6	67	28	67	30	This kind of very general claim definitely needs a reference and might need to be reworded since the whole point of the section (and other discussions on co-benefits/co-costs) is to describe how to identify low-carbon pathways that are consistent with SD goals to avoid the mentioned unintended consequences.	Noted. This section has been significantly restructured with the new draft.
17927	6	67	30	67	36	These sentences resemble some of the key messages of Chapter 4 without any cross-reference (and, indeed, any references). Please liaise with the Chapter 4 LAs to make sure that the results are consistent.	Noted. This section has been significantly restructured with the new draft. References to Ch.4 have been added.
15226	6	68				Figure 6.38 needs to be revised.	Accepted. Replaced with other figure.
15227	6	68				Section 6.7 on Risk. Methodology dealing with risks should be identified.	Rejected. Risk analysis is dealt with in Chapter 2 of the report.
5873	6	68				Figure is not legible, please rework.	Accepted. Replaced with other figure.
5874	6	68	1	70	14	Table can be deleted, information is only referred to in 4 lines in the text (p. 70, l. 11-14) and these lines do not explain why the table should show any risks. Text and table can be deleted without loss of information.	Rejected. The relevance for levels, rate and share of deployment for a risk assessment is explained in the text. The table will be moved to Section 6.6. to allow joint discussion of co-benefits and risk trade-offs.
3150	6	68	1			This chapter has a few figures that could be iconic for the WG3 overall. They include: figs 6.5, 6.6, 6.7 and 6.34. As you trim the chapter pls try to keep those figures and work with TSU to make them clear. For example, add historical data to figure 6.7 to help put the pathways into context.	Noted.
12312	6	68	16			It would be helpful if this section also could focus on how to develop policy strategy and instruments in relation to risk. You might want to coordinate with section 6.3.5 regarding where in chaper 6 this best should be adressed.	Policy instruments are discussed in detail in Part 3 of the WG3 report (Chapter 13-16), and Uncertainty and Risk Management is covered in Chapter 2. A discussion of hedging strategies against risks is more appropriate

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3379	6	69				What are the assumptions of the life-cycle emissions of bioenergy in this table? I suspect that most of the underlying scenarios assume "advanced" = 0 gC02e/MJ. What if it turns out that most realized cost-efficient bioenergy deployment has a notable carbon footprint? Is there place for such a not-first-best-world scenario?	Carbon footprint of bioenergy is include in many integrated assessment model scenarios in the underlying database. ILUC emissions are not an input to these models, but an output. There are also scenarios in the database that do not fully account for ILUC emissions, but they do not show systematically higher bioenergy deployment than the former class of models.
9579	6	69				Please, provide the reason for inclusion ofCCS and lack of hydro with the text or the table.	CCS deployment is relevant for the discussion of risks. Hydro power carries risks too, but models see only small changes in hydro power deployment compared to other low carbon technologies. The focus of the table is on those technologies that see massive changes in deployment in the transformation process.
16743	6	69				Suggest delete. Lacks any context. How derived?	Rejected. The relevance for levels, rate and share of deployment for a risk assessment is explained in the text. The table will be moved to Section 6.6. to allow joint discussion of co-benefits and risk trade-offs. The explanation of how the ranges in the table were derived (from the scenario database) will be strengthened.
7473	6	69				Only so-called modern energy is shown on this table. Why?	The phase out of traditional biomass use is not a main topic of Chapter 6. The foscus of Table 6.7 is on risks due to changes in the supply of modern energy.
3151	6	69	1			This chapter has a few figures that could be iconic for the WG3 overall. They include: figs 6.5, 6.6, 6.7 and 6.34. As you trim the chapter pls try to keep those figures and work with TSU to make them clear. For example, add historical data to figure 6.7 to help put the pathways into context.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16744	6	69	7		18	And what causes the transformation? This seems to lack basis in terms of letting people know how this occurs. Social pressure? Gov't mandates? Appeals to our better nature?	The term transformation refers to the changes in energy amnd land use induced by reducing emissions and mitigating climate change. Climate policy implementation, including its scope and barriers is discussed in Chapter 13-16 of the report.
9906	6	69	9			The Tellus Institute scenarios study referenced above also stresses the need for societal transformation to achieve climate mitigation targets, so this reference should be added here as Raskin, et.al., 2010. See in particular the Great Transition scenario in this paper.	If the general discussion in the paragraph is retained in the SOD, the reference will be added. However, the paragraph may have to be removed due to space constraints.
8697	6	69	9			The Tellus Institute scenarios study referenced above also stresses the need for societal transformation to achieve climate mitigation targets, so this reference should be added here as Raskin, et.al., 2010. See in particular the Great Transition scenario in this paper.	If the general discussion in the paragraph is retained in the SOD, the reference will be added. However, the paragraph may have to be removed due to space constraints.
4770	6	7		7		Yes, waiting for these definitions.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This figure will probably not be ini the ES in the SOD.
18625	6	7				Some sensational statements/conclusions such as: Page 7: Macroeconomic costs for scenarios without CCS and nuclear power are estimated to be as much as two to three times higher than comparable scenarios with full availability of these technologies (all other things being equal).	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement will be revisited
4189	6	7				The interpretation of this figure is not straightforward. One will find a clear relationship and other will not. I would like authors to talk about these figures carefully.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This figure will probably not be ini the ES in the SOD.
5856	6	7				Please make sure you explain "EMF". If the order in the header is "Overshoot … NTE" it should be kept this way in the table lines: "Overshoot" above "NTE".	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This table may not be included in the ES.
5855	6	7				Please make sure you explain "final enegy categories" to the reader.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This figure may not be included in the ES.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9178	6	7		7		it should be noted the costs presented here are estimated based on the assumption that the governmental intervention is cost effective - often it is not the case. As such these are minimum cost estimate.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This table may not be included in the ES.
8617	6	7	1			Why do you include "low-carbon" in the title of the vertical axis? Isn't this the total primary energy supply? What does "low-carbon" refer to"?	Noted.
8618	6	7	1			Where does the text refer to these figures?	Noted.
14451	6	7	1			Axes should start at (0,0).	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This figue may not be included in the ES.
11745	6	7	11	7	13	Clarificaton is needed why macroeconomic costs for scenarios without CCS are estimated to be higher.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement may not remain. If it does, it will be supported by text material within the chapter.
9829	6	7	11	7	13	Please quote the sources for the statement, that the macroeconomic costs for scenarios without CCS and nuclaar power are astimated to be as much as two to three times higher than comparable scenarios. The sentence beginning "For example,….." seems likewise to be false, unless properly qualified. However, there is a new element here which is the relative cost of nuclear power and CCS compared to other no carbon electric generation options. So if the price of nuclear and CCS-related power is more expensive than renewable power options like wind and solar, which many people believe, then not having nuclear and CCS in the mix would actually lower the macroeconomic costs, not raise them. So it all depends, again, on the actual values of key input assumptions, including the price of fossil fuels, as discussed above. So the ranges of input assumptions that yield the result cited must be provided, when making such a sweeping statement. And it must be clear to the reader that with other assumptions the statement would not be true.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement may not remain. If it does, it will be supported by text material within the chapter.
8620	6	7	11			Similarly, the sentence beginning "For example,….." seems likewise to be false, unless properly qualified. However, there is a new element here which is the relative cost of nuclear power and CCS compared to other no carbon electric generation options. So if the price of nuclear and CCS-related power is more expensive than renewable power options like wind and solar, which many people believe, then not having nuclear and CCS in the mix would actually lower the macroeconomic costs, not raise them. So it all depends, again, on the actual values of key input assumptions, including the price of fossil fuels, as discussed above. So the ranges of input assumptions that yield the result cited must be provided, when making such a sweeping statement. And it must be clear to the reader that with other assumptions the statement would not be true.	Please see the response to comment 9829, which appears to be a duplicate of this comment, despite being submitted by another reviewer.
9830	6	7	15	8	2	Mitigation efforts will have an impact on the competitiveness of nations, described in theoretical explanations like the	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9278	6	7	6	7	7	There is no data in the "no CCS" column.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This table may not remain. If it does, it will be improved.
9828	6	7	8			Again, based on my discussion on the cost issue above, I believe this sentence is just plain false. I believe the correct version would be: "All other things being equal, [meaning if all other input assumptions are held constant] the net costs or benefits of mitigation increase disproportionately with increasing stringency of the long-term stabilization goal."	Accepted. We will mention the issue of negative costs. At the same time, the literature we are reviewing overwhelmingly indicates that there will be positive costs.
8619	6	7	8			Again, based on my discussion on the cost issue above, I believe this sentence is just plain false. I believe the correct version would be: "All other things being equal, [meaning if all other input assumptions are held constant] the net costs or benefits of mitigation increase disproportionately with increasing stringency of the long-term stabilization goal."	Please see the response to comment 9828, which appears to be a duplicate of this comment, despite being submitted by another reviewer.
16685	6	7	9	7	13	Do not lose this point -- extremely important for readers to understand that costs increase dramatically if the full suite of technologies can not be deployed based on their relative competitiveness under a carbon price. Should also include the point that costs will likely be much higher than calculated if other policy measures are used instead of a CO2 price to incentivize the deployment of low emitting technologies.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement may not remain.
10981	6	7	11	7	13	The sentence of "macroeconomic costs for scenarios without CCS and nuclear power are estimated to be as much as two to three times higher than comparable scenarios with full availability of these technologies." is good, in terms of indicating the substantial contribution of CCS and nuclear power to mitigation. It should not be deleted.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement may not remain.
16745	6	70	1		5	These seem to me to be extremely different cases -- not clear they hold any lesson for the climate problem in terms of their scope or complexity or means to drive the change needed.	Taken into account. The broad discussion of transformation processes in the section has been reduced and streamlined.
13186	6	70	11	70	11	Should be table 6.7.	Yes. Reference corrected.
16353	6	70	28	70	29	I do not think that the risks of transformation needs to be considered along risks from climate change _alone_, as this sentence suggests. Transformation should be considered in a much broader view: taking into account the benefits and risks from climate change, but also the co-benefits from an integrated transformation to more sustainable societies. Transformation seeking climate mitigation alone is much less justified than transformation seeking sustainability as a whole. Please improve this paragraph and add links with sections discussing co-benefits and sustainable development.	Taken into accout - the discussion of potential adverse side effects
8699	6	70	28			This paragraph should make it clear in conclusion that while of course there will be many risks and serious social disruptions caused by following strong mitigation scenarios such as RCP2.6, the world must do so anyway, because the risks from serious climate change will be far greater. On the other hand, pursuing transformation pathways that actually achieve sustainable development goals will be win-win strategies for humanity.	The IPCC aims to give a broad assessment of mitigation pathways aiming at different levels of mitigation. It is supposed to be policy relevant, but not policy prescriptive.
11434	6	70	36	71	6	There is no reference to policy barriers to technology deployment, transfer and diffusion such as intellectual property rights and other policy instruments. By not including such a reference, the identification of risks to increased technology deployment is therefore incomplete. The IPCC AR5 should be scientific in terms of identifying all possible risks and providing a discussion of these risks.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16747	6	70	40			Rather than "physical resource scarcity" it may be better to say "input price risks …" Scarcity will manifest itself in higher prices. We can typically find supplies but we may not be happy with how expensive they've gotten during times of tight supply or spikes in demand.	Noted.
16746	6	70	6			Use of the word "risks" … do we mean costs? Or uncertainty?	The term risk will be clearly defined in the AR5 glossary, and Section 6.7 will adhere to this. The introduction of the section will focus on clarifying the type of risk addressed in the section (the risk of mitigation failure) and distinguish it from risk trade-offs (Chapter 6.6) and risk analysis (Chapter 2).
17929	6	70	17	70	17	The inclusion of environmental side-effects in a risk discussion is not consistent with agreements reached in Wellington by which environmental side-effects will be framed as either co-benefits or co-costs of mitigation policies/actions and technical and operational risks would be discussed seperately. If this should not be appropriate for specific kind of side-effects (e.g. page 71, lines 3-4), this concern should be raised during LAM3.	The discussion of potential adverse side-effects has been moved to Section 6.6 Sustainable Development where it will be discussed together with co-benefits. This includes environmental side effects.
18641	6	71				Page 71: The next draft may discuss shares of “fluctuating renewables” in relation to grid integration. Refer to our studies and the IEA study	Noted. The discussion of supply technology risks is mostly done in Chapter 7, and relevant parts are now summarized in Section 6.6. Thus, the discussion of technology risks will be moved to Section 6.6 from 6.7.
9580	6	71	1	71	5	These parts have biases for nuclear; please, take into account following and reflect some in the text; Abram and Ion describe the International Generation-IV Initiative which was established with the aim of fostering the research and development necessary to underpin the development of a new generation of nuclear energy systems. These Generation-IV systems, which comprise both the reactors and their associated fuel-cycle facilities, are intended to deliver significant advances compared with current advanced light water reactors in respect of economics, safety, environmental performance, and proliferation resistance. The Generation-IV systems are expected to be developed to the point of commercial deployment by at least 2030. The Generation-IV International Forum (GIF) members have identified six reactor systems that offer the potential for meeting the Generation-IV goals. (T. Abram and S. Ion (2008) Generation-IV nuclear power: A review of the state of the science, Energy Policy 36 (2008), See downloaded file “Abram Ion 2008.pdf”)	Rejected. Risks of nuclear energy are treated evenly with risks of other energy technologies. The discussion of supply technology risks is mostly done in Chapter 7, and relevant parts are now summarized in Section 6.6. Thus, the discussion of risks of Nuclear energy will be moved to Section 6.6 from 6.7.
6412	6	71	13	71	13	"status quo bias" is a behavioral decision-making term that doesn't really apply here. I understand the intent of the sentence, and it seems to me as though reference to path dependence is more appropriate here.	Noted.
13187	6	71	21	71	21	The reference is incorrect; the authors of the cited paper are Strachan and Usher.	Taken into account. Reference corrected.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16749	6	71	31		47	Problems of the intersection of energy security vs. food security are not the result of climate policy per se but rather the misguided attempt to push preferred technologies without having given thought to the life-cycle carbon emissions associated with the technology. These problems could be partly avoided through better accounting of carbon as well as not mandating particular technologies (bio-fuels for transport in this instance).	Noted.
4208	6	71	36	71	40	Most important societal risk is "unemployment" especially in the transition period.	Noted.
16748	6	71	9		30	Perhaps this can be simplified to say that as some technologies increase their share of the market, they may cause increases in system operating costs not reflected in the costs of an individual project. As written seems overly complicated.	Noted.
17930	6	71	31	71	32	Please liaise with Chapter 7 to provide a consistent assessment of the challenges of fluctuating renewables and whether this should be framed as a risk (rather than an institutional and technological barrier).	Fluctuating renewables can pose a risk to energy system reliability and thus are an adverse side effect and risk trade off. Now delegated to Section 6.6 that will attempt to summarize risks of energy technologies from Chapter 7.
15934	6	71				It may be worth discussing the water footprint of the energy technologies available for use today - as they will in a very large part determine the water use intensity of the world's mitigation strategies (in an ideal world ofcourse) - which is an important part of any comprehensive solutions set.	The water footprint of mitigation technologies is a potential adverse side effect and risk trade-off and will now be discussed in Section 6.6 Sustainable Development together with co-benefits. (see Response to Comments 933 and 953).
16750	6	72	11		20	Would be helpful to report how large an increase in commodity food prices and how this impacts the very poor more than rich (who consume more processed food and for whom food budget is smaller share of household budget). How can we mitigate these impacts and where does it matter?	Noted. We agree that information on food price increases are highy relevant, and the relevant literature is referenced more extensively. The discussion has been moved to Chapter 6.6.
16751	6	72	39		47	This should be using the outputs from integrated models which rely on carbon price -- the various chapters do poor job of laying out least cost to most costly options and are very difficult to line up, especially when comparing across sectors. So the question is, if author relies on the various chapters, how do they line up the options and the associated costs? What do you assume drives the transformation? This is important -- if it is a Co2 price that rises over time, we don't know precisely what techs will deploy where, but we know the transformation will occur if market participants are confident in policymakers long term commitment. If driven by individual country's policy mandates, then more effort may be required to ensure there are not unintended consequences or large amounts of emissions' leakage.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
17932	6	72	26			The use of the terms 'public acceptability' is inconsistent with agreements reached in Wellington (p.36). In accordance with chapter 2 usage, the term 'public perception' would be preferrabe (even to 'public acceptance' on page 71, line 6).	Changed to public perception

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16752	6	73	22		46	I don't understand the value of this discussion. Is it needed to articulate the potential technology deployment under a CO2 price? Or is it to define for policymakers those policies or standards they should implement? Models can only roughly show whether a path is possible and the relative costs of different pathways -- they are imperfect. As the future unfolds, we learn and adjust. Models can not foresee the details or reliably chart every future change.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
11757	6	73	36	74	2	Delete to save the volume. These seem to be needless.	The misuse of the bottom-up/top-down terminology in the literature illustrates that it is important to stress this point. However, we will make an effort to shorten the disucssion to a minimum.
9582	6	73	36	74	2	Please, delete here or move to footnote.	The misuse of the bottom-up/top-down terminology in the literature illustrates that it is important to stress this point. However, we will make an effort to shorten the disucssion to a minimum.
6413	6	73	6	73	21	There seems to be too much summary of AR4 and not enough direct articulation of the important evolution in AR5.	Discussion of comparison between sectoral and integrated studies in AR4 will be shortened.
11756	6	73	6	73	21	Delete to save the volume. These seem to be needless.	Discussion of comparison between sectoral and integrated studies in AR4 will be shortened.
9581	6	73	6	73	21	Please, delete here.	Discussion of comparison between sectoral and integrated studies in AR4 will be shortened.
9419	6	74				There are so many lines and it is difficult to understand this figure and its explanation.	The lines have no meaning and were not part of the original document. There seems to have been a conversion error in the preparation of the FOD.
16753	6	74				How is this useful to policymakers? Better information might be: 1) how big is energy use in buildings, can it be reduced via market price on carbon for less than similar reductions in the electric sector? The goal should not be zero-net-energy use by buildings, but rather reducing emissions at the lowest possible costs. If zero net energy use is less costly than zero emissions from other sources, they should happen first. If the cost of zero net use is more costly, it should happen much later. Economic modeling and experience show that carbon price is most efficient and effective -- help policymakers understand how building energy use responds in that context.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9907	6	74	2			One of the major bottom-up studies that has been done with a great degree of sectoral disagregation is the Raskin, et. Al. 2010 study of four scenarios referenced above. (Sustainability 2010) This study provides far more detail than is described in sections 6.2 and 6.3 of this chapter. The Tellus Institute paper is supported by a 300 plus page Technical Documentation of the Polestar model on the Tellus website www.tellus.org. The link is: http://www.tellus.org/publications/files/TheCenturyAhead_TechDoc.pdf	The suggested publication will be considered for inclusion in the SOD. However, as pointed out by the reviewer, parts of Section 6.3 are probably the place where this information should be incorporated in the first place.
8700	6	74	2			One of the major bottom-up studies that has been done with a great degree of sectoral disagregation is the Raskin, et. Al. 2010 study of four scenarios referenced above. (Sustainability 2010) This study provides far more detail than is described in sections 6.2 and 6.3 of this chapter. The Tellus Institute paper is supported by a 300 plus page Technical Documentation of the Polestar model on the Tellus website www.tellus.org. The link is: http://www.tellus.org/publications/files/TheCenturyAhead_TechDoc.pdf	The suggested publication will be considered for inclusion in the SOD. However, as pointed out by the reviewer, parts of Section 6.3 are probably the place where this information should be incorporated in the first place.
6497	6	74	9			6.8.2.1 (Sectoral Energy Use Industry, Transport, Human Settlement) and 6.8.3 (Regional (Sectoral) Analysis and Transformation Pathways Industry, Transport, Human Settlement) had better to be aggregated. Because they are similar in the content.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
9583	6	75	16	75	18	Please, replace here with following; public acceptance is one of the common and major obstacles that should be solved when building new power facilities.	Comment is noted and will be considered as the text is adjusted for the next draft.
9584	6	75	20			Please, add the following information; developing infrastructure such as transmissions for new plants take a long lead-time, therefore, it is indispensable to solve the institutional issues for infrastructure development and particular barriers for uncertainty for policy implementation. (ECORYS (2010) “Assessment of non-cost barriers to renewable energy growth in EU Member States “) http://ec.europa.eu/energy/renewables/studies/doc/renewables/2010_non_cost_barriers.pdf ECORYS [1] identified the nine major issues and ranked these issues in order of severity, divided over three groups. According to the study, “most severe types of barriers” include: 1) Administrative hurdles like planning delays and restrictions, lack of coordination between different authorities, long lead-times in obtaining authorizations, severe costs for obtaining permission, etcetera. 2) Barriers linked to grid connection and access affecting all RES-E technologies, are the second main obstacle - not so much in terms of the physical connection (where administrative and cost issues dominate), but limited priority access with regard to fossil power production, insufficient transport capacity linked to obsolete infrastructure, and limited interconnection capacity may block or at the least delay renewables development. 3) Issues related to limited information and awareness include a lack of general knowledge on RES benefits, poor dissemination of support measures, poor knowledge dissemination of pilot and/or demonstration projects and insufficient funding for awareness campaigns.	This point is based on the discussion in the energy systems chapter (Chapter 7) and will be resolved in accordance with the discussion there.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13281	6	75	22	75	34	As mentioned earlier, CCS is a key option for decarbonisation in a number of important industry sectors. This is especially true for those sectors, such as cement and iron&steel, that cannot be fully decarbonised with renewables or nuclear, as some of their CO2 output results from chemical reactions	This point has been added in the discussion of economics of mitigation. Depending on space availability we will hihglight this again and/or cross-reference to that seciton.
16754	6	75	22		34	This is not that helpful. What is the significance? In a carbon constrained world with a carbon price, these sectors or commodities would factor in the carbon price. As the price increased, the commodity price or activity costs would increase driving innovation to find substitutes, change processes to reduce emission intensity or otherwise respond to reduce this part of their cost structure.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
16030	6	75	26			another 30% or more	This point is based on the discussion in the industry chapter (Chapter 10) and will be resolved in accordance with the discussion there.
16755	6	75	36	76	6	Is the goal in a climate policy to reduce energy use or to reduce carbon emissions? Much of this discussion is based on engineering studies which ignore or overlook many market realities which are not easily dealt with even via a policy. The policy should be emissions focused. The building sector efficiently participates in lowering emissions by responding to the carbon price signal embedded in the delivered energy -- to go beyond this usually means inefficiency -- which is likely unsustainable.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
15013	6	76	12	76	16	Compared with the description on LDV, this part of aviation is too long.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15115	6	76	12	76	16	The statement in this paragraph is not accurate. In 2010, the 37th Session of the Assembly of ICAO endorsed among other things: (1) a global aspirational goal of 2 per cent annual fuel efficiency improvement up to year 2050 ;(2) a medium term global aspirational goal from 2020 that would ensure that while the international aviation sector continues to grow, its global CO2 emissions would be stabilized at 2020 levels and (3) develop a global CO2 Standard for aircraft aiming for 2013.	The FOD version of Section 6.8 is based on an ad-hoc review of the sectoral chapters with an attempt to compare the findings presented in these chapters with the developments in the transformation pathways assessed in Chapter 6. The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
16757	6	76	23			Recent research re electric vehicles shows that most owners charge their vehicle at home. There is not the need for the massive charging infrastructure frequently cited. You might find these helpful: • Idaho National Labs (which helps run the EV Project – a DOE initiative funded by ARRA): http://avt.inl.gov/index.shtml, http://avt.inl.gov/pdf/EVProj/EVProjInfrastructureQ22012.pdf • UC-Davis (preeminent research institution on PEV driver behavior): http://phev.ucdavis.edu , http://pubs.its.ucdavis.edu/publication_detail.php?id=1470 (groundbreaking study on driver behavior) □	The FOD version of Section 6.8 is based on an ad-hoc review of the sectoral chapters with an attempt to compare the findings presented in these chapters with the developments in the transformation pathways assessed in Chapter 6. The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
16767	6	76	23			Re time needed to make infrastructure changes for fuel switch by transport to electricity -- this is true if discussing moving from vehicles to rail perhaps, but if discussing vehicle fuel switch to electricity, there have been several studies that show this may be relatively easy w/out the massive infrastructure change -- most people charge (or will charge) their electric vehicle or plug in electric hybrid vehicle at home in the evening. This is supported by work cited here: • Idaho National Labs (which helps run the EV Project – a DOE initiative funded by ARRA): http://avt.inl.gov/index.shtml, http://avt.inl.gov/pdf/EVProj/EVProjInfrastructureQ22012.pdf • UC-Davis (preeminent research institution on PEV driver behavior): http://phev.ucdavis.edu , http://pubs.its.ucdavis.edu/publication_detail.php?id=1470 (groundbreaking study on driver behavior) □	The FOD version of Section 6.8 is based on an ad-hoc review of the sectoral chapters with an attempt to compare the findings presented in these chapters with the developments in the transformation pathways assessed in Chapter 6. The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15014	6	76	24	76	26	Need to stress that these options are not easy to implement.	Comment is noted and will be considered as the text is adjusted for the next draft.
17135	6	76	25			DELETE: modal shift REVISE TO: well harmonized multi-modal transport.	Comment is noted and will be considered as the text is adjusted for the next draft.
16253	6	76	41	76	42	I would be hesitant to call spatial planning a "holistic approach".	This point is based on the discussion in the human settlements chapter (Chapter 12) and will be resolved in accordance with the discussion there.
8047	6	76	7	76	11	This view on transport is too much car centered. We know OECD countries like Japan where public transport has a market share of 50 %. Of course cars are mostly the biggest source of emissions. But in the beginning of this chapter there should not be the car (as the main problem) but the transport structure with its diversity.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
16756	6	76	8		26	Looking at aviation -- fuel is already the largest costs component, so carefully managed. A carbon price embedded in the fuel should be adequate to create economically efficient emission reductions. Producing separate targets based on engineering potential are likely driving uneconomic reductions and distracts policymakers from the true least costs approach. Renewable fuel standards, using bio-energy via liquid fuels, done outside of a comprehensive carbon market that includes land use changes, risks large increase in emissions via increased deforestation.	This point is based on the discussion in the transport chapter (Chapter 8) and will be resolved in accordance with the discussion there.
8048	6	77	23	77	28	We know that the cost ($/t CO2) in the transport sector are often higher that in other sectors (with this logic not much should be done in the transport sector). But we know the multiple co-benefits if we reduce CO2-emissions in the transport sector (e.g. modal shift leads to better air, less accidents, livable cities) which often are more important that the value of the CO2 saved. Conclusio: to look only on CO2 is not helpful, mention also the many other co-benefits.	Co-benefits of climate mitigation will be discussed in Section 6.6.
16758	6	77	30		40	What are the economics of high density vs. low density development? Why are some cities high density and others low density? There is utility in both -- how increase utility/desirability of high density? If CO2 cap includes emissions from land use changes, electricity, industrial activity and direct emissions from fossil fuels, how are cities in different context likely to evolve? Has this been examined anywhere to compared to current development trends?	This point is based on the discussion in the human settlements chapter (Chapter 12) and will be resolved in accordance with the discussion there.
16254	6	77	31	77	31	The reference (Müller et al 2011) is wrong: this article has not been published yet (will be submitted in September 2012 to Science).	Reference will be updated.
5233	6	77	6			Emissions from energy conversion are not considered here separately, although a different logics is applied in the chapter 6.8.2.1.	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4210	6	77				Will LCA approach be touched upon here?	The comparison of sectoral and integrated mitigaiton studies is still work in progress and therefore the entire Section 6.8 is under revision with more information from both sets of studies becoming available. The section is therefore being restructured for the SOD.
9420	6	78				There are so many lines and it is difficult to understand this figure and its explanation. It is more informative to analyze relations between sectoral energy use and sectoral CO2 emissions. When discussing sectoral CO2 emissions, it is imporant to clarify whether effects of electricity savings in the demand side are included in the demand side or such electricity saving potentials in the demand side are counted in the Power sector.	The lines have no meaning and were not part of the original document. There seems to have been a conversion error in the preparation of the FOD.
18642	6	79				A box on page 79 discusses mitigation wedges and and MACs. Underlines the methodological problems. No one has ever claimed that it is anything else that a very simplistic way of presententing choices to be made (in relation to an assumed BAU, statical and doesn't mirror dynamics) but it can still be a relevant way to present that there are alternatives laying at our feet. What is the alternative? Expecting that policymakers and the general public should understand the full dynamics?	The box is supposed to create awareness of the methodological problems that some methods frequently used in policy relevant studies have.
16759	6	79	2		10	Thank you for relating this back to a carbon price.	You are welcome.
16760	6	79	35		46	Good points!	Thank you.
7474	6	79	6	79	8	"The mitigation options differ greatly by activity, regions, system boundaries and the time horizon. Forestry mitigation options - including reduced deforestation, forest management, afforestation, and agro-forestry - are estimated to contribute between 1.27 and 4.23 Gt CO2/yr [0.35 and 1.15 Gt C] abatement in 2030 ---". These are very low figures, especially when the accessible NPP for woody biomass is about 27 Gt C – 98 Gt CO2. This is over 20 times the CO2 estimate for 2010! Thus, using more fully the annual NPP of trees will more than satisfy this meager target.	This point is based on the discussion in the AFOLU chapter (Chapter 11) and will be resolved in accordance with the discussion there.
18626	6	8				Page 8: Technology alone will not stabilize greenhouse gas concentrations.	Noted.
12306	6	8	13	8	13	Please insert "and sinks" after "emissions".	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement may not remain. If it does, the comment is noted.
9177	6	8	16		21	You must refer to SRM - even if you stabilize consentration you may have high climate change impacts. SRM has to be developed as insureance	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. This statement may not remain. Regardless, the desire to bring forward SRM is noted.
12623	6	8	19	8	21	Bioenergy and CCS is a very valid technology but may be constrained by the availability of sustainable biomass. This must be taken into account when estimating the infultration of bio CCS into any overshoot scenario.	Noted.
12666	6	8	19	8	21	Bioenergy and CCS is a very valid technology but may be constrained by the availability of sustainable biomass. This must be taken into account when estimating the infultration of bio CCS into any overshoot scenario.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4771	6	8	20	8	20	Please explicit acronyms: CCS "carbon capture and storage", and CDR "carbon dioxide removal"	Editorial.
13126	6	8	20	8	20	Write out CDR, as it appears here for the first time.	Editorial.
2183	6	8	23	8	43	The ClimateWorks Australia Low Carbon Growth Plan for Australia (and its 2011 update) should be included in the database. They can be accessed by authors and reviewers at http://www.climateworksaustralia.org/publications.html	Noted. There is an open call for scenarios to support this synthesis. The developers of the highlighted scenario are encouraged to submit it to the database.
9834	6	8	23	8	36	Where is the database available for the reader? Please provide information. This is also very important concerning the issue on assumptions raised above. The report gains credibility when the public has the possibility to access at least part of the information.	Noted. The database will be made available upon completion of the final draft.
9831	6	8	3			I think you mean "Changes in technology" or "Improvements in technology…" to the degree they are incorporated into baseline scenarios, as the next sentence makes clear.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified.
9832	6	8	3	8	21	Life style has a huge impact and the willingness-to-accept on real-world feasibility and should be considered.	Noted.
13125	6	8	3	8	6	Although I do not seriously doubt this conclusion, I don't think it can be made based on the model outcomes. In other words, technology alone will not bring emissions down unless one expects technologies capable of doing this to emerge and implements them in the model (i.e. low cost, carbon free, high potential technologies. A very optimistic fusion scenario, for example). The current observation is somewhat circular and just indicates that virtually nobody has created such a scenario. One can naturally speculate that this is because such a scenario would seem "unlikely", but one could equally well argue that it's "likely" that the 2100 energy system has aspects that would today be consider "unlikely" - or ones we are currently know nothing about. Qualify the conclusion better.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified
11369	6	8	3	8	5	The statement "Technology alone will not stabilize greenhouse gas concentrations" would preferably need a short reasoning on 'why'. The following two sentences read also very 'isolated'. This should be better connected and justified.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified
16686	6	8	3			Insert "improvements absent a CO2 price" after "Technology …" at start of sentence.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified
8621	6	8	3			I think you mean "Changes in technology" or "Improvements in technology…" to the degree they are incorporated into baseline scenarios, as the next sentence makes clear.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified
9835	6	8	38			Trade-offs in transition pathways are a very important issue that should be considered more in depth and positioned at a more prominent part of the report.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9836	6	8	44	10	15	Writing the report from a normative scenario perspective and adding a backcasting approach would increase the possibility to raise the awareness of decision makers, like in this sentence. This perspective could enrich the whole report. The questions given in this part should be positioned in a more prominent place of the report and used as guidelines for structuring. Moreover it might encourage decision-makers to think in options and longterm consequence	Noted.
3072	6	8	44	8	46	Same comment as p. 5 lines 3 and 29, above	See response to previous comment.
4190	6	8	44	8	44	"dangerous" -- Does it mean "determinstic" or "probabilistic" or both? It seems to me, "possible dangerous" would be better expression. Otherwise, it sounds as if "zero-emission" could avoid all climate risks.	Noted. This phrase may not longer be found in the introduction in the new revisions. Regardless, whether it remains or a similar statement remains in the introduction, we will no longer include a reference to dangerous anthropogenic interference with the climate.
13216	6	8	44	8	44	In the past, IPCC has always carefully avoided to express an opinion on what is "dangerous", considering that it was a political matter, not a scientific one. Please, don(t use this word when qualifying the most severe emissions reduction discussed by the policy makers	Noted. This phrase may not longer be found in the introduction in the new revisions. Regardless, whether it remains or a similar statement remains in the introduction, we will no longer include a reference to dangerous anthropogenic interference with the climate.
14394	6	8	46			“Co2 emissions … must eventually be brought to or below zero.” This is flat out wrong. According to IPCC SAR4, Scientific, p. 512, there is a natural exit of 3.3 GtC (12.1 GtCO2) from the atmosphere annually. So at stabilization there could be new emissions of at least this amount, not requiring going to zero. Since most abatement cost models show extremely non-linear cost curves as emissions are cut toward zero, it is misleading and unduly pessimistic to assert that emissions must be cut to zero. My figure for the required target for CO2 emissions to achieve 450 ppm stabilization is 1.4 tCO2 per capita per year.	Noted. The introduction is being revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified
13127	6	8	46	9	1	I assume this comment has been left in accidentally?	Noted.
14452	6	8	46			CO2 emissions can never be zero, all animals respire CO2 and organic matter decomposes. Do you mean net fossil-fuel and LUC derived CO2 emission must be zero? This concept must be edited for accuracy.	Noted. The introduction is being revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified.
15426	6	8	46	9	1	It's not clear how /if this author comment will be integrated in the text; in our opinion, it should not be integrated in the text; if it is integrated, it should in no way weaken the statement that CO2 emissions must be brought down to or below zero. No credible scientist has suggested that the existence of geoengineering would change the need to dramatically and immediately reduce CO2 emissions -- in fact, scientists involved in geoengineering research have made assurances that geoengineering SHOULD NOT and WOULD NOT dilute or detract from mitigation efforts.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
14393	6	8	7			Need to translate 2.6 W/m2. Using 0.3°C/W-2 as the direct warming effect and 1.9 as the multiplier to get total including feedback, each W-2 translates to 0.57°C (with climate sensitivity at 3°C for doubling). So the target of 2.6 represents an extremely ambitious limit of 1.5°C. The usual 2°C limit would be 3.5 W-2. That is the border between category 2 and 3. Are the authors trying to insert an unusually ambitious goal through the back door?	Noted. At a climate sensitivity of 3 W/m2, long-term equilibrium RF of 2.6 Wm2 is equivalent to 2 degrees of warming. At the same time, the transient temperature could be very different.
9833	6	8	7			This sentence raises other critical issues. This line says "Many integrated models are unable to produce scenarios …". What I think this sentence should say to be properly qualified is "Given the sets of input assumptions utilized in some integrated assessment models when running the 2.6 W/m2 stabilization scenario, including limits on the availability of certain supply-side and higher efficiency end-use technologies..." Again, part of my point is that the models are not the problem, it's the input assumptions that yield certain results given the model structures.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified
8622	6	8	7			This sentence raises other critical issues. This line says "Many integrated models are unable to produce scenarios …". What I think this sentence should say to be properly qualified is "Given the sets of input assumptions utilized in some integrated assessment models when running the 2.6 W/m2 stabilization scenario, including limits on the availability of certain supply-side and higher efficiency end-use technologies..." Again, part of my point is that the models are not the problem, it's the input assumptions that yield certain results given the model structures.	Please see the response to comment 9833, which appears to be a duplicate of this comment, despite being submitted by another reviewer.
11371	6	8				Despite being an introduction section, references to literature are required for certain statements, where especially also 'literature' is being mentioned	Noted.
6905	6	8	44	8	46	Missing reference to support this rather strong statement (actually true for the entire section!). Please refer to WG AR5 (or to the WGI TAR or the WGI AR4) for the physical science basis to support this statement.	Noted. Referencing will be improved in the SOD.
6536	6	8	46			Replace "be brought to or below zero" with e.g. "be reduced significantly" in accordance with AR4 WG1 Report Figure 10.21, or give a reference paper.	Noted. The introduction is being revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified.
6904	6	8	16	8	18	Suggest to refer to WGI AR5, Ch12, in relation to stabilization, allowable emissions, and projected climate change in the underlying assessment supporting this statement.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If such a statement remains, it will be linked to WGI in the main text rather than the ES.
10982	6	8	18	8	19	The reasons why "pathways increased flexibility in the near-term implies deeper reductions in the long-term" are not clearly stated.	Noted. The ES is being substantially revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, the reader will be able to understand its basis by reading the chapter.
2426	6	80	10	80	13	This is a weird statement. One can always make a confident summary of current understanding, even if the understanding is low. I can confidently say that we don't know about something. Or do the authors mean something else? I would question the statement that there is a "deep body of policy analytic literature" on iron fertilization.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15430	6	80	14			DELETE: "usefully" -- subjective (rhetorical) adjective is unnecessary and, in this context, inappropriate.	text completely revised, comment no longer applies
8510	6	80	15	80	15	What is “geosphere”? May be “lithosphere”?	text completely revised, comment no longer applies
8511	6	80	18	80	18	“Biochar” is not a “technology”. A reader can understand this slang if a list of methods and their characteristics is presented previously.	The term "methods" was decided upon
2424	6	80	20	80	42	I think the case for iron fertilisation is somewhat overdone. It is not clear why more than a third of the section is devoted to iron fertilisation given the current uncertainties on C export to the deep ocean.	In the final presentation iron fertilization receives a more proportianate coverage with a little over one paragraph.
2425	6	80	20	81	9	This text should cross-reference Chapter 6 of WGI assessment. In fact, most of it could be removed if Chapter 6 is cross-referenced, which would free up some space to discuss WGIII specific issues on CDR.	The section was rewritten and connections to the relevant parts of the other chapters and other WG reports was made
13740	6	80	20	80	20	It is possible (add:) to a certain degree...	text completely revised, comment no longer applies
15431	6	80	21		42	What is missing from this summary is the assertion by scientists that iron fertilization, as a climate change response strategy, should be abandoned. See, for example, A. Strong, J. Cullen, and S. W. Chisholm. (2009) Ocean Fertilization: Science, Policy, and Commerce, in Oceanography: Vol. 22, No. 3, 236-261 and Strong et al., "Ocean fertilization: time to move on," Nature 461, 347-348 (17 September 2009) | doi:10.1038/461347a, published online 16 September 2009 and CBD Technical Series 45, "Scientific Synthesis of the Impacts of Ocean Fertilization on Marine Biodiversity," 2009. Because of possible negative impacts and the lack of scientific justification for pursuing it as a climate change response, iron and other forms of ocean fertilization have been subject to a de facto moratorium in the Convention on Biological Diversity since 2008; the moratorium was strengthened in 2010 and reaffirmed at the Rio+20 UNCSD 2012 conference. (Rio+20 outcome document, "The Future We Want," 2012, para 168: "We stress our concern about the potential environmental impacts of ocean fertilization. In this regard, we recall the decisions related to ocean fertilization adopted by the relevant intergovernmental bodies, and resolve to continue addressing with utmost caution ocean fertilization, consistent with the precautionary approach." [online] http://www.uncsd2012.org/thefuturewewant.html)	The authors avoided such strong policy recommendations and instead focused on identifying limits and issues of the various methods.
15432	6	80	24			INSERT: "intended" -- i.e., "the intended net effect"	text completely revised, comment no longer applies
2427	6	80	25	80	25	"a given ATMOSPHERIC input of fossil carbon"	text completely revised, comment no longer applies
15433	6	80	26			INSERT A NEW SENTENCE after "…fossil carbon:" "The assumption that this would result in permanent sequestration has been challenged." See, for example, A. Strong, J. Cullen, and S. W. Chisholm. (2009) Ocean Fertilization: Science, Policy, and Commerce, Oceanography: Vol. 22, No. 3, 236-261 and Strong et al., Nature 461, 347-348 (17 September 2009) | doi:10.1038/461347a, published online 16 September 2009.	text completely revised, comment no longer applies
2428	6	80	27	80	27	"mass ratio"	text completely revised, comment no longer applies
8512	6	80	28	80	28	Sulfur is a macro-component of sea salt. It is not considered as a critical nutrient for marine biota. It cannot be used for “ocean fertilizing”. May be “sulfur” is mistakenly used instead of “phosphorous”?	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15434	6	80	29		30	INSERT after: costs are expected to be low (Shepherd et al. 2009): "though disruptions to the marine ecosystem, including the marine food web, are expected to be significant." (A. Strong, J. Cullen, and S. W. Chisholm. (2009) Ocean Fertilization: Science, Policy, and Commerce, in Oceanography: Vol. 22, No. 3, 236-261 and Strong et al., "Ocean fertilization: time to move on," Nature 461, 347-348 (17 September 2009) | doi:10.1038/461347a, published online 16 September 2009 and CBD Technical Series 45, "Scientific Synthesis of the Impacts of Ocean Fertilization on Marine Biodiversity," 2009)	the following is included in the brief paragraph outlining ocean iron fertilization: "There are a number of possible risks including downstream decrease in productivity, expanded regions of low oxygen concentration and increased N2O emissions (See WGI Section 6.5.3.2) (low confidence).". as this section must cover many methods briefly we felt that this was sufficient and was covered by 6.5.3.2 of WG1.
9908	6	80	4			I would either omit section 6.9 or greatly shorten it. The grounds for omitting it are that geoengineering technologies have never been carefully integrated into climate mitigation scenarios, and certainly not by IAMs. Furthermore the lack of knowledge of the physics, chemistry, and economics of geoengineering schemes are so great at this time as to make most discussions of these possibilities almost pure speculation.	The literature on solar geoengineering now spans many hundreds of papers published over many decades. That literature in turn rests on a body of scientific knowledge of climate that is substantially the same as the body required to understand the climate impacts of greenhouse gases and aerosols. If discussion of geoengineering is pure speculation then we would have to concede (falsely) that discussion of greenhouse gas driven climate change was pure speculation.
8701	6	80	4			I would either omit section 6.9 or greatly shorten it. The grounds for omitting it are that geoengineering technologies have never been carefully integrated into climate mitigation scenarios, and certainly not by IAMs. Furthermore the lack of knowledge of the physics, chemistry, and economics of geoengineering schemes are so great at this time as to make most discussions of these possibilities almost pure speculation.	Same as previous
8509	6	80	4	80	5	The title of section 6.9 might be unclear for a reader. What is “carbon”? What “radiation” should be managed? What should include “environmental risks”? It could be better to use the following title: “Geo-engineering approaches to prevent global warming”.	the title distinguishes between the different approaches to geoengineering due to their heterogenity. The need to distinguish different approaches due to the very different potentials and risks they pose is a key message of the section. Terms are clarified at the beginning of the section and in the glossary.
8052	6	80	4	82	39	In this paragraph on geoengineering I miss the political aspect on some of the drivers. We see the fossil fuel industry as a driver - there some who have been climate deniers for a long time, they did not want mitigation, and now they say it is too late for mitigation we have to go to geoengineering.	This is not the place for political analysis.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3147	6	80	4			section 6.9 (on geoengineering) sticks out and doesn't belong here. What does geoengineering have to do with transition pathways?	Answer: ??? (I personally agree that it makes little sense to have geoengineering in this chapter but it is a decision of the IPCC.)
8050	6	80	42	80	42	the wording 'with a wide variety of potential benefits and impacts' sounds too positive having in mind the 'large-scale disruption to ecology of the ocean'. Write at least 'with a wide variety of potential benefits and large negative impacts'	text completely revised, comment no longer applies
16761	6	80	43	81	21	This is very clearly written and logical -- helpful!	text completely revised, comment no longer applies
13739	6	80	7	80	7	Insert before "a diverse": " As global emissions continue to surpass the expectations deployment of carbon negative technologies can be seen as a requirement to stay below 2 degrees of warming in this century (de Elzen et al., 2012)."	text completely revised, comment no longer applies
3285	6	80	4			The two subsections (6.9.1 and 6.9.2) take very different angles to approach the two broad types of geoengineering (CDR and SRM) and would benefit from harmonization.	No specific suggestions for harmonization were provided so it is hard to respond.
8508	6	80	4			In section 6.9 (between 6.9 and 6.9.1) the main principles of geo-engineering should be presented: (a) definitions of SRM and CDR; (b) difference between SRM and CDR; (c) goals of SRM and CDR implementation; (d) conditions of beginning and stopping of SRM and CDR implementation (timescale of implementation); (e) potential efficiency of different geo-engineering methods (or potential forcing). The objective of geo-engineering formulated in 6.9 should correspond to formulations given in the Introduction: “to prevent abrupt or catastrophic damages which can be provoked by possible climate crisis”. It should be kept in mind that geo-engineering of CDR type deals with GHG removal ONLY FROM THE ATMOSPHERE (not from smoke gases in the industry).	We agree that more clarity about timescale would be beneficial. The link tying geoengineering to abrupt climate change is too strong, this is but one of a set of arguments advanced for the possible use of geoengineering.
6285	6	80	5	80	6	One outcome of the IPCC Experts Meeting on Geoengineering which was held in Lima in 2010 was that there would be a statement to the effect: "While the term "geoengineering" is used in some discussions of what can be done to address climate change, that term does not have a specific scientific or technical meaning that is understood across many different research cmmunities. Therefore, this section will discuss the two distinct research topics of "Carbon Dioxide Removal" and "Solar Radiation Management" that are often lumped under this broader term of "geoengineering." This could serve as a short block of text that separates the boldfaced headers for Sections 6.9 and 6.9.1.	Yes.
6290	6	80		81		A general note on section 6.9.1, this section is pretty light on references to the peer reviewed literature. For key points, I would suggest citing more (perhaps many more) peer reviewed papers. For example, there is a large literture about the potential role of BECCS and more than just the Wise et al, 2009 paper should be cited here. The short paper by Dooley in the IPCC Geoengineering Experts meeting cites a number of these papers on the role of BECCS and some of those peer reviewed paper (and not the Dooley summary) should be cited here.	OK.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6287	6	80	11	80	13	The authors of Chapter 6 need to figure out what should be covered in Section 6.9. If the focus of Section 6.9 is narrowly defined to be what does the peer reviewed literature tell us about the role of CDR and SRM in "Transformation Pathways" then I think this sentence ought to read that it is only for BECCS that there is a robust literature about how SRM and CDR play in Transformation Pathways. Yes there is a literaure about iron fertilization but it is an engineering, ecological and earth system science focused literature and not a literature about how iron fertilization fits into a portfolio of potential responses to cliamte change. That is a question for the authors of Chapter 6 is should there be a clear emphasis about what is known and more importantly not known about how CDR and SRM fit into "Transformation Pathways" and less a description of the basic technologies absent this sense of how they fit into a broader set of actions? Also any sentence that says there is a sufficiently deep body of scientific and policy analytic literature ought to have way more than two citations.	True, there is little in the literature on transformatoin pathways
6289	6	80	20	81	3	There is nearly a page devoted to ocean fertilization but at the end the reader has no better understanding of how does this concept of ocean fertilization fit into "Transformation Pathways." Is ocean fertilization 100% of the solution to anthroprogenic climate change or 0.01%? Is it really cheap and therefore something that would be done early (assuming a Hotelling like price path for GHC emisssions to the atmosphere) or is it very expensive? I am not suggesting that Chapter 6 needs to answer these questions but I do think the authors and review editor(s) for Chapter 6 neeed to think about wheter in the context of Chapter 6 or WGIII's contribution to AR5 it is better to devote a page telling the reader about the basic biological and biogechemical processses involved in ocean fertilization. Or is it better to point the reader to good technical literature that describes the potential processes for iron fertilization and then devote the text here to making it clear that before AR6 comes around there is a pressing need to understand how this class of CDR activities would fit into "Transforamtion Pathways." Compare the text on ocean fertilization to the text two paragraphs down about BECCS and DAC. BECCS and DAC are also complex systems but the reader is (appropriately) not walked through whether it is better to use NaOH or something else to capture CO2 from the air or the specifics of the configuration of the DAC units or other critical technical details. For the purpose of this chapter the discussion of DAC, BECCS and biochar seem to be at the right level and hit the points that are relevant for a discussion of "Transformation Pathways."	In the final report iron fertilization receives a more proportianate coverage with a little over one paragraph.
4309	6	80	45	81	3	the possibility to add alkalinity to the oceans might be workig in theory, but in practice it poses a large challenge. thus, it is not only risky and expensive, but also highly impractical (see Borel, B. (2008): Cleaning up CO2 with a twist of lime. Cosmos Magazin. http://www.cosmosmagazine.com/node/2117)	The paragraph on enhanced weathering notes the idealized nature of some studies and points to a number of risks and limitations and concludes that "The confidence level on the carbon cycle impacts of enhanced weathering is low" and that further details are provided in WG1 6.5.2.3
4311	6	80	6	81	21	all CDR-approaches involving BE-CCS or DAC have the same storage-obstacles common CCS has. (see IPCC special report on CCS). That has to be noted in this particular section to make it more balanced.	the final report notes: "Carbon captured through CCS, BECCS and DAC are all intended to use the same storage reservoirs (in particular deep geologic reservoirs), potentially limiting their combined use under a transition pathway.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6919	6	80	6			This section mostly focuses on the assessment of the physical science basis of specific geoengineering options. However, this component of the assessment of Geoengineering is done in WGI AR5 and a reassessment here in WGIII Ch6 must be avoided to avoid unnecessary overlap and potential inconsistency within the WG AR5 assessment. Rather than producing your own assessment, reference to WGI AR5, Chapter 6 should be made for a comprehensive assessement of the physical science basis of CDR. We suggest to also consider the cross-WG IPCC Expert Meeting Report on Geoengineering held in June 2011 (IPCC, 2012: Meeting Report of the Intergovernmental Panel on Climate Change Expert Meeting on Geoengineering [O. Edenhofer, R. Pichs-Madruga, Y. Sokona, C. Field, V. Barros, T.F. Stocker, Q. Dahe, J. Minx, K. Mach, G.-K. Plattner, S. Schlömer, G. Hansen, M. Mastrandrea (eds.)]. IPCC Working Group III Technical Support Unit, Potsdam Institute for Climate Impact Research, Potsdam, Germany, pp. 99.).	This section has been rewritten and expanded. It was decided to gives an accessible overview of the physical scence before exploring the other relevant issues. Sections 6.9.1.2, 6.9.2.2 and 6.9.3 give this broader perspective
6286	6	80	7	80	11	Consider moving the sentence about there being many different CDR techniques and we don't know that much about how they would play out in practice up into the short section suggested to separate Section 6.9 and 6.9.1 and make it a more general statement about CDR and SRM. Then cite the many reports that stress this point that there are lots of potential ideas but few if any have really been fleshed out or tested in the real world (we have nuclear power plants and we have half a century of opperational data from them, there is no comperable body of knowledge for anthroprogenic CDR or SRM schemes). This point needs to be front and center before the individual technologies are discussed. IPCC, Meeting Report of the Intergovernmental Panel on Climate Change Expert Meeting on Geoengineering, O. Edenhofer, et al., Editors. 2012, IPCC: Potsdam, Germany. p. 108 Vaughan, N.E. and T.M. Lenton, A review of climate geoengineering proposals. Climatic Change, 2011. The Royal Society, Geoengineering the climate: Science, governance and uncertainty, 2009, The Royal Society: London. p. 98. Asilomar Scientific Organizing Committee, The Asilomar Conference Recommendations on Principles for Research into Climate Engineering Techniques, 2010, Climate Institute: Washington DC. p. 40.	The final report notes carefully the limits to current knowledge on these methods.
6288	6	80	7	81	21	At the IPCC experts meeting on geoengineering there was considerable discussion about not throwing everything under the header of CDR. In particular, there was push back as to whether things like no till agriculture, afforestation, and potentially even BECCS should be included under CDR. The reason for not including changes to agricultural practices and forestry under CDR is that these are already well developed concepts with their own literature and even accounting rules and that nothing is gained by including them under CDR/geoengineering. I think I would leave DAC, BECCS, biochar and ocean fertilization in this section 6.9 but I would seriously consider removing much of the "traditional" terrestrial management things from this discussion.	Yes
2429	6	81	1	81	1	I assume you mean "atmosphere" rather than "biosphere" here.	text completely revised, comment no longer applies
8051	6	81	10	81	16	This sounds as if this technology had no problems with acceptance. We know the SR on CCS and the optimistic expectations, but since then much of the optimism on CCS projects has vanished. Please reflect shortly that BECCS could meet the same problems, and the question where the huge amount of biomass would come from is equally an open one.	These issues are covered in the final draft.
2430	6	81	12	81	14	Is this "summary" still up to date?	This section has been rewritten, updated and expanded.
2431	6	81	14	81	16	The sentence contradicts itself: the cost of BECCS is similar to coal with CCS although the cost of biomass is unrelated to coal. I suspect you mean the cost of the CCS is similar for BECCS and for a coal-fired power plant. However the two technologies achieve different things: coal+CCS produces (almost) carbon-free energy, BECCS produce energy and (ideally) withdraws carbon from the atmosphere.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15436	6	81	14			After "comprehensive summary." INSERT A NEW SENTENCE: "However, safe and permanent storage of CO2 is a major hurdle; leaked CO2 could have significant negative impacts (Shaffer, 2010)." See Gary Shaffer, "Long-term effectiveness and consequences of carbon dioxide sequestration," Nature Geoscience, 3, 464 – 467 (2010) Published online: 27 June 2010 | doi:10.1038/ngeo896	text completely revised, comment no longer applies
14337	6	81	23	81	25	The text emphasises the "fact" that SRM would act quickly. However, recent studies havemitigated this prospect. Cf. Williamson, P., Watson, R.T., Mace, G., Artaxo, P., Bodle, R., Galaz, V., Parker, A., Santillo, D., Vivian, C., Cooper, D., Webbe, J., Cung, A. and E. Woods (2012). Impacts of Climate-Related Geoengineering on Biological Diversity. Part I of: Geoengineering in Relation to the Convention on Biological Diversity: Technical and Regulatory Matters. Secretariat of the Convention on Biological Diversity. Montreal, Technical Series No. 66	Most of a paragraph is now devoted to the issues of geological storage.
13741	6	81	23	81	23	Rephrase: "The feature that makes SRM special for climate policy is the very quick response of climate variables upon its successful deployment (Shepherd et al. 2009)."	text completely revised, comment no longer applies
8515	6	81	23	81	23	"Role" cannot act quickly or slowly. SRM methods can.	text completely revised, comment no longer applies
8514	6	81	23	81	24	It is appropriate to add two references: Budyko, 1982 (Budyko, M.I. 1082. The Earth's Climate: Past and Future. New York: Academic Press) and Izrael, 2005 (Izrael, Yu.A. 2005. An efficient way to regulate the global climate is the main objective of the solution of the climate problem. Russian Meteorology and Hydrology, No. 10, pp. 1-4)	text completely revised, comment no longer applies
15437	6	81	23			Section 6.9.2. should be considered for deletion due to the speculative and controversial nature of SRM. At the very least, the section needs an introduction that conveys the speculative / theoretical nature of SRM, such as the following: At the beginning of line 23, INSERT: Blocking or reflecting sunlight away from the earth (so-called Solar Radiation Management) is a controversial proposition because it has the potential to cause significant environmental damage, including releasing additional GHGs into the atmosphere, changing weather patterns (including reducing rainfall), damaging the ozone layer, diminishing biodiversity, reducing the effectiveness of solar cells, and risking sudden and dramatic climatic changes if the efforts are stopped, either intentionally or unintentionally. SRM will not address the problems of atmospheric GHGs or ocean acidification and could even worsen ocean acidification and ozone depletion. (Robock A., Oman L. & Stenchikov G. [2008]. Regional climate responses to geoengineering with tropical and Arctic SO2 injections., J. Geophys. Res., 113, D16101, doi: 10.1029/2008JD010050.) Political questions are equally critical: Who controls the Earth's thermostat? Who will make the decision to deploy if such drastic measures are considered technically feasible and with whose consent? If something goes wrong, who is responsible for the damages? (See ETC Group, Geopiracy, The Case Against Geoengineering, 2010 [online] http://www.etcgroup.org/content/geopiracy-case-against-geoengineering)	SRM was included in the final report given the growing literature base and public and policy awareness. The final report is substantially altered from the earlier draft form and much care has been taken to note technical uncertainties and risks but also the socio-political and ethical issues that SRM raises.
8516	6	81	26	81	26	SRM methods do not "mask". Cooling effect caused by them counteract warming effect caused by GHGs.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13742	6	81	28	81	29	Rephrase: "Emissions reductions result in mitigation of climate change on time-scales of more than decades because of the inertia inherent in the carbon cycle. On the century timescale, however, only the reduction of emissions and sequestration of GHGs can reduce the long-run climate risk; SRM might provide rapid cooling for only both a limited time and limited level of GHG concentrations. ..."	We do not think this text makes sense because it (falsely) condones acceptance of the idea that SRM would be used to provide rapid cooling. Since many climate impacts depend on the rate of temperature change it is hard to imagine any circumstance under which it would make sense to use SRM for rapid cooling.
8517	6	81	30	81	30	It would be better to add the word “concentrations”: …only reduction of concentrations of long lived GHG can reduce…	text completely revised, comment no longer applies
2423	6	81	34	81	43	This paragraph does not bring much and could be deleted if space is needed.	text completely revised, comment no longer applies
8961	6	81	34			This claim is not true. Public understanding, as well as technical understanding is minimal.	This claim is no longer made
13743	6	81	34	81	34	Rephrase "Scientific understanding and public understanding of SRM is, though growing rapidly, still very limited (Shepherd et al. 2009); (Mercer et al., 2011)."	"Very" limited is a value judgment. It is objectively true that with a doubling time of about two years the growth in literature is rapid.
8962	6	81	38		44	Scientiic opinions should be weighed, not counted. Joseph Henry ca. 1850. The claims here are meaningless.	text completely revised, comment no longer applies
8518	6	81	43	81	43	It is appropriate to add a separate paragraph: “Along with theoretical investigations (Izrael et al. 2007 – Izrael Yu.A., Borzenkova I.I., and Severov D.A. 2007. Role of stratospheric aerosols in the maintenance of present-day climate. Russian Meteorology and Hydrology, No 10, pp. 1-4) some limited field experiments related to SRM were conducted lately in Russia (Izrael et al. 2011 – Izrael Yu.A., Zakharov V.M., Ivanov V.N. et al., 2011. Field experiment to simulate influence of aerosol layers on changeability of solar insolation and meteorological characteristics of the atmospheric boundary layer. Meteorology and Hydrology, No 11, pp. 5-14, in Russian). The results of the experiments to study interaction of solar radiation with artificial aerosols and natural cloud layers demonstrated that reduction of radiation flux led to relatively fast reaction of temperature and turbulent heat fluxes within atmospheric boundary layer. It was shown that partial screening of the surface by aerosol can be considered as effective means for control of solar radiation intensity and temperature regime of air boundary layer. There are broad prospects for international cooperation to carry out field experiments of different scale under the auspices of World Meteorological Organization.	WG1 covers more of the technological and physical science basis and as such WG3 is not the appropriate venue for this issue.
8519	6	81	44	81	48	The effectiveness of a geo-engineering method is its capability to compensate (partly of fully) positive radiative forcing caused by GHGs. The effectiveness of SRM and inability of SRM to compensate effects of GHGs perfectly on regional scale are absolutely different things. The effectiveness of space-based and stratospheric-based RGM methods are not limited on global scale (Lenton T.M. and Vaughan N.E., 2009. The radiative forcing potential of different climate geoengineering options. Atmos. Chem. Phys. Discuss., vol. 9, pp. 2559-2608; The Royal Society, 2009. Geoengineering the climate: Science, governance and uncertainty. ISBN: 978-0-85403-773-5, 83 p.). Indeed, theoretically it is possible to compensate perfectly any change of a climatic parameter (global averaged) by SRM but due to different physical basis of greenhouse warming and SRM regional distribution of the parameters will be different.	We do not believe this citation materially as to the understanding of geoengineering, so we propose not to include it on account of the space constraints.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
15435	6	81	9			At end of line 9, INSERT: In all cases, permanent (or even medium-term) sequestration has not been established. (A. Strong, J. Cullen, and S. W. Chisholm. (2009) Ocean Fertilization: Science, Policy, and Commerce, in Oceanography: Vol. 22, No. 3, 236-261 and Strong et al., "Ocean fertilization: time to move on," Nature 461, 347-348 (17 September 2009) | doi:10.1038/461347a, published online 16 September 2009 and CBD Technical Series 45, "Scientific Synthesis of the Impacts of Ocean Fertilization on Marine Biodiversity", 2009)	This section notes a number of limitations of ocean iron fertilization and raises the issue of the longevity of carbon storage in general however section 6.5.3.2 covers these specific issues in more detail.
4310	6	81	14	81	16	Biomass is currently a decentral form of energy while CCS necessarily requires a centralized system. The comination of both has so far only been on a theoretical level and migh in practice require a different mode of biomass-“production“ (large plantations, infrastructure...). This has an impact on costs and overall CO2-mitigation levels (UBA 2011, S.23, Aznar, c., Lindgren, K., Larson, E. & Möllersten, K. (2006): Carbon capture and storage from fossil fuels and biomass – costs and potential role in stabilizing the atmosphere. Climatic Change, Volume 74, Numbers 1-3 / Januar 2006, S. 47-79.)	BECCS is covered alongside other CDR techniques and a number of limitations are noted both in section 6.9.1.1 and 6.9.1.2 but are covered in more detail in section 6.5.1 of WG1
6291	6	81	20	81	22	It is probably true that the Socolow APS study of DAC is the only broad-based study and I think that study is accurately described here, however this is an IPCC assessment of the peer reviewed literature and I think peer reviewed papers on the cost of DAC also ought to be cited. Zenz House, K., et al., Economic and energetic analysis of capturing CO2 from ambient air. PNAS, 2011. Baciocchi, R., G. Storti, and M. Mazzotti, Process design and energy requirements for the capture of carbon dioxide from air. Chemical Engineering and Processing, 2006. 45(12): p. 1047-1058. in additon to work produced by Keith et al and Lackner et al should probably be cited here too.	The report has been updated and a broader range of literature has been cited. Section 6.5.1 of WG1 covers this particular issue in more depth.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
9585	6	81				Please, take into account the following information in the text; The other key geo-engineering mechanism, Solar Radiation Management (SRM), is suggested as a low-cost climate change intervention tool that may temporarily alleviate climate change. More is known about SRM as scientific research into the field continues and research programs are sponsored. Nevertheless, the inherent efficacy of SRM remains limited as it cannot perfectly compensate for the effects of the proliferation of green house gases. A critical climate issue is the serious problem of the decrease in the pH of the Earth's oceans. The increased levels of anthropogenic CO2 in the atmosphere have already caused significant ocean acidification during the past decades and the rate of ocean acidification is ever increasing [1]. Hoegh Guldberg et al., in an academic paper on climate change effects on coral reefs [2], project that ocean acidification will bring about an oceanic pH drop of 0.4 pH units by the end of this century, with ocean carbonate saturation levels potentially falling below levels necessary to sustain coral reef accretion by 2050. An IPSO backed expert workshop summary report [3] contends that acidification increases the susceptibility of corals to bleaching, changes the behaviour and toxicity of heavy metals and may reduce the limiting effect of iron availability on primary production in some parts of the ocean. Consequently, as a result of the above, the environmental and societal impact is clearly huge. Stabilizing and turning around the effects of climate change on the oceans is a long-term task, according to the National Oceanic and Atmospheric Administration (NOAA) chief Jane Lubchenco [4]. The United Kingdom's Parliamentary Office of Science and Technology published a short briefing note on geo-engineering research in 2009 [5]. It mentions the significant uncertainty intrinsic in models predicting SRM effects. The note hinted that climate outcomes not foreseen by modelling might arise and, importantly, it also states that SRM 'has no effect on the other consequences of elevated CO2 levels such as ocean acidification.' The publication goes on to report that solar radiation management techniques 'can do nothing except buy time for efforts to reduce atmospheric CO2 to succeed as they do not address the root causes of climate change.' Similarly, the IPCC Meeting Report of the Intergovernmental Panel on Climate Change Expert Meeting on Geoengineering [6] emphasises that whilst SRM may impact precipitation patterns on a regional basis, SRM by itself offers no substantial solution for CO2-induced ocean acidification. Likewise, in a paper published by the Royal Society, it is said that SRM techniques will not address effects caused by increased concentrations of atmospheric greenhouse gases, such as ocean acidification [7]. Therefore, it may be prudent to include such an inherent shortcoming in the draft section on SRM and its related capabilities and limitations. [1] Gangjian Wei, Malcolm T. McCulloch, Graham Mortimer, Wengfeng Deng and Luhua Xie. (2009) Evidence for ocean acidification in the Great Barrier Reef of Australia. Geochimica et Cosmochimica Acta. Volume 73, Issue 8, Pages 2332–2346 http://dx.doi.org/10.1016/j.gca.2009.02.009 [2] Hoegh-Guldberg, et al. (2007). Coral Reefs Under Rapid Climate Change and Ocean Acidification. Science. Vol. 318 no. 5857 pp. 1737-1742 http://dx.doi.org/10.1126/science.1152509 [3] Rogers, A.D. & Laffoley, D.d'A. 2011. International Earth system expert workshop on ocean stresses an	This comment makes a substantial number of very reasonable points, but almost all of them are already addressed in the text.
8399	6	81				Since this section focuses on risks, the risk due to cessation of SRM should be mentioned, a risk that is proportional to the amount of SRM used. (Coordinated with Chapter 13, which also discusses SRM)	In the revised section the risks of SRM are covered in greater detail than in the previous draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3286	6	81	22			This subsection completely ignores the literature on other types of SRM than stratospheric aerosol injections. While the stratospheric injections have been the most extensively studied method, there is a considerably body of literature especially on cloud brightening but also on surface albedo modification (there are too many publications to be listed in detail here; they can be easily found with little effort). I also find the cited literature biased toward a couple of North American research groups, and e.g. the European studies on the feasibility and effects of different SRM methods are ignored. In addition, some of the major risks related to SRM methos (such as termination effects, ocean acidification, potential hydrological changes related to monsoon circulation and rainfall in the Amazon region, etc.) merit more discussion.	The section has been rewritten and now covers a range of SRM methods which are covered in more detail in section 7.7.3.
6920	6	81	22			This section mostly focuses on the assessment of the physical science basis of specific geoengineering options. However, this component of the assessment of Geoengineering is done in WGI AR5 and a reassessment here in WGIII Ch6 must be avoided to avoid unnecessary overlap and potential inconsistency within the WG AR5 assessment. Rather than producing your own assessment, reference to WGI AR5, Chapter 7 should be made for a comprehensive assessement of the physical science basis of SRM.	Answer: we will ensure that better coordination with WG1.
6292	6	81	23	81	24	Suggest striking the first sentence here and moving the citations at the end of the current first sentence to the end of the current second sentence. Both sentences say the samething but the current second sentence is more informative and uses more technically precise terminology "decades" as opposed to "quickly."	text completely revised, comment no longer applies
6293	6	81	28	81	29	Suggest striking sentence that beggins with "Emissions mitigation neceearily…" This point about the relative time scales of mitigation and SRM is made at least three times in this opening paragraph. This can be said once. Also the repetition of this point makes it sound as if these things are substitutes as opposed to commpliments. In terms of SRM and mitigation being compliments, I'd suggest citing a number of papers that make this point. A recent addition to the literature on this point is Smith SJ and PJ Rasch (2012) The Long-Term Policy Context for Solar Radiation Management Climatic Change (accepted).	We agree this point should only be made once.
6294	6	81	29	81	30	While true, the sentence that says "Mitigation cannot…" seems a bit inconsistent with the discussion earlier in this chapter about how near term actions or inaction drives the shape of longer term options. Again, the point here should not be whether SRM or mitigation is better or faster or whatever other metric but rather that they represent potential commpliments but that there really isnt a literature describing this complementary role in terms of "Transformation Pathways" and that really is somethign that needs to be developed before AR6	The reason they are potential compliments is that the have differing capabilities.
4312	6	81	31	81	31	this sentence ignores the well acknowledged „moral hazard“ problem. therefore you might insert „...misconception to think of a simple climatic one-time trade-off between“	the moral hazard problem is raised in the final report
8403	6	81	32	81	33	I'm not sure that the paper cited actually talked about tradeoffs, although Goes et al. explicitly look at this and should probably be referenced. Also, our recent paper shows that, if SRM was needed, then SRM and mitigation would need to occur together in order to reduce both near- and long-term risks. References: Smith, Steven J and PJ Rasch (2012) The Long-Term Policy Context for Solar Radiation Management Climatic Change (accepted). Goes M, Tuana N, & Keller K (2011) The economics (or lack thereof) of aerosol geoengineering. Climatic Change 109:719–744 DOI 10.1007/s10584-010-9961-z.	We will look at the new Smith and Rasch paper.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6295	6	81	34	81	35	The Shepherd et al 2009 paper is not listed in the references so this reviewer can not determine to what extent that paper supports the assertion that the science SRM and public support for SRM has "grown rapidly." The Mercer 2011 article is listed in the references and can be found on line. It is not clear how the Mercer paper supports the assertion that "public understanding of SRM is growing rapidly." The Mercer paper includes time series data on the "publics expososure" to news stories about SRM but that is clearly not the same as understandng. The Mercer paper cites unpublished previous studies that show public awareness of SRM is potentially less than 10%. Malone et al 2010 based on the pioneering work of Bishop et al 1986 argued that surveys with response rates that low are likely measuring non-stable psuedo opinions. The Mercer paper states "We found that the assessed familiarity of geoengineering is likely around 8%, which is greater than past empirical assessments." and notes that this is a single assessment. Again, I do not see any evidence that "public support for SRM" has grown rapidly. Delete this concept from AR5 which is meant to assess the collective wisdom of the peer reviewd literature and not one or (depending on what is in the Shepherd paper) two papers. Malone, E., J. Dooley, and J. Bradbury, Moving from misinformation derived from public attitude surveys on carbon dioxide capture and storage towards realistic stakeholder involvement. International Journal of Greenhouse Gas Control, 2010. 4(2): p. 419-425. G.F. Bishop, A.J. Tuchfarber, R.W. Oldendick Opinions on fictitious issues: the pressure to answer survey questionsPublic Opinion Quarterly, 50 (1986), pp. 240–250	This text will either be removed or altered to ensure that there is no implication that support is growing rapidly. The point that does seem supported by data is that knowledge is growing rapidly.
6296	6	81	34	81	43	Delete this entire paragraph. It is not clear what the point of this paragraph is supposed to be. This section should either discuss how SRM fits into Transforamtion Pathways or describe key technical / scientific points. The history of SRM literature and how it grew from a tiny litterature to a slightly larger literature isn't a core point.	this paragraph has been removed and the final version of the report does focus on these issues.
6297	6	81	44	82	13	This is well written and informative. I think that this text here as well as a shortened version of the first paragraph in Section 6.9.2 is probably all that needs to be said about SRM in terms of Transformation pathways. I would end this section with a paragraph stressing that much work needs to be done to understand the role of SRM and CDR in Transforamtion Pathways (i.e., society's potential responses to climate change) and then be done with it. [i would certainly cite the Moreno-Cruz et al 2012 paper but probably drop the sentence that describes the particular metric used in that study. the text in the proceeeding sentence that says "but (c) one of the first studies to examine the effectives geoengineering in compensating for temperature or precipitation changes on a regional basis shows that SRM can compensate for increased GHG surprisingly well even at a regional level" is adequate discription of the Moreno-Cruz et al 2012 work for the purpose of chapter 6 in WGIII [WG1 or WGII would seem better places to discuss the specifics of this study].	The text has been rewritten and around 3 pages are devoted to SRM. Many methods for SRM are described and a range of risks and uncertainties deiscussed as well as the potential role for SRM in transformation pathways. This longer format was deemed necessary to appropriately cover this controversial issue.
8513	6	81	22			Section 6.9.2 deals with only one version of SRM namely injection of submicron aerosol into the stratosphere. It should be mention that reflection of a part of solar radiation can be provided by different surfaces in different locations. So, at least 4 types of SRM should be distinguished: space-based, stratospheric-based, cloud-based, ground-based. Shortly they are mentioned in sections 9.5.2 and 13.4.2. Besides, the text of 6.9.2 focused mainly on shortcomings of stratospheric-based SRM. Nothing is said about physical principles of SRM. The main part of the text of the section is devoted to discussion of shortcomings of SRM. It is one-sided approach. There is no ideal method to prevent global warming. Any conclusion about advantages and disadvantages of this or that method must be done on the basis of cost-benefit analysis.	Many methods of SRM are now described in this section and their limitations noted.
8963	6	82	13		16	This is a very crude model at best. Too much emphasis given to it in the report.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13695	6	82	20	82	20	Add text after "productivity": "SRM may lead to unwanted changes in regional climate patterns such as the monsoons, with the potential of massive damage (Burns 2011, Keith et al. 2010)." References: Burns, W. (2011): Climate Geoengineering: Solar Radiation Management and its Implications for Intergenerational Equity, in: Stanford Journal of Law, Science & Policy, 4, p. 39-55; Keith, D.; Parson, E.; Morgan, G. (2010): Research on global sun block needed now, in: Nature, 463, 426-427	The risks of SRM are raised and covered in more detail than in the earlier draft.
9244	6	82	21	82	39	SRM measures could change the precipitation patters and reduce direct solar radiation. The former has potentia impacts to water resource. The latter has explicit impacts to solar thermal energy potential.	The text makes this point.
5234	6	82	23			At least two types of risks could be mentioned also. First, the risks related to the governance of SRM systems (refer e.g. to Chapter 13.4.2) and, second, the risk related to ocean acidification if the atmospheric CO2 concentration is not limited but the radiative forcing is compensated by SRM (see e.g. FOD of WG I or the IPCC Workshop on Impacts of Ocean Acidification on Marine Biology and Ecosystems, Okinawa, Japan, 2011).	It make sense to make some mention of the difficulties (or risks) associated with governance. If we mention ocean acidification we simply need to mention that SRM does not reduce its risk. It is not, however, correct to call ocean acidification risk of SRM.
8964	6	82	24		25	Ozone and, I would say hydrological impacts, which indicate reduced rainfall in Africa and India.	The risks of SRM are raised and covered in more detail than in the earlier draft.
8522	6	82	24	82	25	It would be too naïve to say that “Ozone depletion … is by far the best studied risk”. The experimental data obtained just after the Pinatubo eruption shown that ozone depletion within the volcanic cloud was significant (up to 20%). However, nobody proved scientifically that such depletion was caused by sulfuric acid droplets (used for SRM) but not by volcanic ashes (not used for SRM). It was evaluated that Pinatubo produced global ozone depletion on the level of 2.5% (Kinnison D.E., Grant K.e., Connell P.S., Rotman D.A., Wuebbles D.J., 1994. The chemical and radiative effects of the Mount Pinatubo eruption. J. Geoph. Res., Vol. 99, No D12, pp. 25705-25731, doi:10.1029/94JD02318). It is important to remember that Pinatubo injected into the stratosphere about 10 Mt of sulfur that is much more than would be needed for SRM purposes.	There are far more papers about the ozone depletion risk of solar geoengineering then there are about any other geoengineering risk.
8523	6	82	25	82	28	The phrase “For sulfate aerosols…” is contradictory: (a) additional aerosol reduces NOx; (b) reduced NOx should produce less ClO; (c) reduction of ClO leads to preservation of ozone. The net result is: the higher aerosol concentration, the higher ozone concentration	The text has been revised and the ozone consequences of sulphate aerosol injections are discussed briefly. WG 1 section 7.7.3 covers this issue in greater depth
8965	6	82	36		38	This reduced loading claim is very uncertain and is based on wishful thinking.	text completely revised, comment no longer applies
8520	6	82	4	82	5	“cycle” cannot decrease. Evaporation or precipitation amount can.	text completely revised, comment no longer applies
17397	6	82	6	82	16	The following study should be mentioned: Riche, K.L., Morgan, M.G., Allen, M.R., 2010. Regional climate response to solar-radiation management. Nature Geoscience 3: 537-541.	Other citations which address similar issues are cited in the report.
8521	6	82	8	82	8	Double “that”	text completely revised, comment no longer applies
6298	6	82	21	82	39	I would delete this as a stand alone paragraph and instead take the literature cited here and summarize it in a closing paragraph agbout unknowns and future research. This seems too detailed for this chapter.	The text has been completely rewritten and greater space is devoted to unknowns and future research

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
16762	6	83	11		20	This seems much more important when policymakers are trying to define what must happen via policy rather than when simply setting a carbon constraint with a price and letting the market figure it out.	Noted.
13744	6	83	14	83	14	Insert "The apparent availability of carbon negative or SRM technologies will also influence mitigation strategies."	Noted. We will consider including the treatment of SRM as a priority in the next draft upon consideration of the chapter team.
2230	6	83	21	85	16	Those 3 questions are (the) essential questions the world asks the IPCC on mitigation. Hence, carefully considered answers are needed, which are as clear as possible on the answers. Do a cross check across all chapters to avoid any duplication of those FAQ (e.g, currently there is a Copenhagen Accord target question in chapter 7 which shoudl not be there (but is rather a duplication of this Q1 (6.11.1))	Noted.
2231	6	83	22	83	24	The question should be reformulated, e.g. with respect to "chances to achieve the Copenhagen Accord, i.e. stabilizing at max 2 degrees warming). A phrase like "under control" is too vage.	Rejected. At present, we are comfortable with more general language that would be clear even to a non-expert.
11372	6	83	25	83	25	This sentence needs to be reformulated to be clearer	Accepted.
16763	6	83	4		10	It is important to highlight that economic response, in terms of technology choices, infrastructure development and so on will vary from place to place depending on current level of development. Price based policies allow for this and result in each country following the most efficient pathway via trade of emissions. It is extremely difficult for governments or analysts to pre-define the best pathway and then to build policies to make the future conform to the analysis.	Noted.
16764	6	83	42			Suggest inserting after "across countries" the following: "whether they rely on more or less costly or effective approaches,"	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
16765	6	83	44			At end of paragraph, suggest inserting: "Research indicates that policies which use a carbon price to incentivize change are the most effective and least costly means to lower emissions of GHGs. The timing with which a market is developed that includes at minimum the largest emitting countries is a major determinant as to whether or not a 450 ppm or 500 ppm target can be realistically met." [It is generally realized that absent an agreement with trading of emissions between the major emitting countries that the goal of 2 degrees is impossible -- I think this should be said in the document or we can not expect policymakers to know it.]	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
2232	6	83	45	84	7	Can the stabilization target be expressed (also in) temperature warming. Radiative forcing values will just not be used and understood outside the climate scientists community.	Rejected. This chapter is focused on RF. We will note the link to temperature early in the chapter.
11373	6	83	45	84	2	Also this sentence does not read well. Is there some part missing? Please consider to reformulate that.	Accepted.
13746	6	84	10	84	10	Rephrase: "There are many technologies that can contribute to reducing the carbon intensity of human activities. This means that no single technology can serve as a "silver bullet", …	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
4211	6	84	11	84	14	The term "risk" should appear in this sentence to clarify the relationship with 6.7.	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
11758	6	84	14	84	17	Good example.	Noted.
9586	6	84	14	84	17	Good comment and good for exective summary	Noted.
6760	6	84	14		17	Good description. It's very important.	Noted.
10652	6	84	14	84	17	Appropriate statement.	Noted.
5875	6	84	14	84	17	I object. There is no such thing as a zero-carbon electricity source. Nuclear power installations cause emissions during construction, maintenance, running, intermediate and / or final waste storage, fossil energy with CCS just reduces efficiency to c&s C which "is there" (and this source also has emissions from construction, running and maintenance), and "renewables" of course also have C emissions! Low or "zero" emissions during the "electricity generating phase" have to be related to the life-cycle emissions of the "electricity generating device" to get the complete assessment.	Noted. It may be wise to move to a nomenclature on low-carbon technologies.
13747	6	84	17	84	17	Insert: "In addition to reducing the carbon intensity of energy systems the contribution of carbon negative technologies such as bio energy with carbon capture and storage (BECCS) will be invaluable to counteract diffuse sources of carbon such as land-use change induced emissions or emissions from transportation and to reach adequate emission reduction pathways despite upcoming implementation difficulties. …"	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
11374	6	84	19	84	19	Why mention here only technology? Also changed behavior (in not using too much energy) could be mentioned here, even the question adresses technology. This somehow implies that technology might be the 'sole' solution	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
13748	6	84	19	84	19	Insert after "… will": "also"	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
13745	6	84	2	84	3	Rephrase: " Indeed studies indicate a global emissions peak a requirement for this goal, … (den Elzen et al., 2012)"	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
11375	6	84	23	84	25	This statement implies that 'only' technologies would be able to overcome this issue; here again, changed human behavior (in theory) could be an alternativ.	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
13749	6	84	23	84	29	Delete	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
8966	6	84	24		25	Analysis of technology is woefully lacking, since tech can be a two-edged sword and can generate its own dynamics.	Noted.
16031	6	84	26	84	29	i not agree with this argumentation. First: CCS is not enough prooved. And why is this the only way? What is with bioenergy and use of CO2 for other purpuses for example with algae. What is with the production of renevable energy per wind or solar and the production of hydro or methan with the electricity that is not used (Power to Gas-Technology)	Rejected. The literature on scenarios shows the potential benefits of CDR technologies. We do not assert here that the technology is available or proven, only that it would be valuable it it were.
8053	6	84	26	84	26	insert 'is': 'The one possible exception to this is biomass coupled with carbon dioxide capture and storage.'	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
9588	6	84	27	84	29	Please, describe the reality of BioCCS here as it may have limitation to deploy and uncertainty as follows; Rhodes and Keith in a 2008 peer-reviewed commentary on biomass with capture noted that while the high end of estimates for potential biomass availability support the view that biomass could provide the central mechanism for managing global climate and energy challenges, it is doubtful because [1] of the deep uncertainty in the feedstock supply estimates; the environmental implications of maximizing production; the complex social and ethical issues arising from the required re-organization of global land use; and the potentially high costs of such a strategy. They further note that [2] relatively large allocations of land in the developing world would be required to support the scales of bio-energy development implied by globally-aggressive biomass-based strategies. For example, land availability estimates indicate that 84% of arable land not in commercial use is in tropical regions of the world. Local food production capacity, which likely represents a more immediate concern in the developing world than carbon emissions, could be displaced. More generally, rural populations could be forced to adapt to radically changed local environments, including environmental consequences from large-scale biomass production. The notion that these disruptions should be absorbed by the developing world in order to mitigate carbon emissions in industrialized nations raises complex ethical issues of “biomass justice”. [1] J.S. Rhodes and D.W. Keith (2008) Biomass with capture: negative emissions within social and environmental constraints: an editorial comment, Climatic Change, 87, p. 323, lines 9-14. [2] J.S. Rhodes and D.W. Keith (2008) Biomass with capture: negative emissions within social and environmental constraints: an editorial comment, Climatic Change, 87, p. 323, lines 31-41.	Noted. This literature will be considered in the discussions of CDR technologies.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
2234	6	84	30	85	16	Repeated comment from above which is true here as well: The IPCC should consider to contrast the sum of three cost elements to society when presenting this UNDER DIFFERENT SCENARIOS: 1) Mitigation, 2) Adaptation, 3) Damage cost. Then it will get obvious, that with more money invested in mitigation the TOTAL cost to society can actually be kept lowest. (Dentist analogy). The current representation of JUST showing cost for mitigation only, has of course the consequence that the more mitigation you are doing, the more cost you will incur. Consequently, mitigation is seen as MAIN cost to society, while the other cost elements will likely be bigger and will have much higher uncertainty.	Rejected. This is not the job of this chapter. This chapter is just focused on mitigation.
11995	6	84	30			I would suggest to compare the cost of mitigation to the cost of extreme weather events, rising sea levels etc.	Rejected. This is not the job of this chapter. This chapter is just focused on mitigation.
16766	6	84	31		47	Can you make point that these are costs only, not net cost or benefits. Presumably, we have made determination one way or another that the possible downsides are costly enough that we want to act. Policymakers and the public also interpret costs in terms of absolute loss, usually from what they have now. Suggest we help translate costs so people understand this is really a slight reduction in growth rates -- that economies still grow over time, and that people's welfare continues to improve. Can translate costs into additional time needed to achieve same level of GDP/capita in the no policy case.	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
2233	6	84	8	84	29	1) It should be noted that the solution is very fragmented across sectors and technology options, thus clearly a "silver bullet" does not exist. 2) Do include some non-CO2 examples (too energy focused now), 3) Energy efficiency measures play a too small role in the answer (too much power supply), 4) Consider to systematically walk through all sectors with 2-3 examples of mitigation options each.	Noted. This section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
10998	6	84	14	84	17	Fossil energy with CCS might not be said as a zero-carbon electricity source. In the chapter 5, nuclear power is described as "near zero-carbon electricity source". The sentence of "nuclear power and fossil energy with CCS are not sufficient without technologies such as heat pumps and electric cars that can allow electricity to substitute for liquid and solid fuels" could lead to misunderstanding. To make use of nuclear energy and renewable energy instead of fossil energy contributes enough to mitigate greenhouse gas emissions.	Noted. It may be wise to move to a nomenclature on low-carbon technologies.
17478	6	85	14	85	14	should this say "increase costs" rather than "reduce costs"?	Accepted. However, this section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
6543	6	85	14			"increase cost" instead of "reduce cost"?	Accepted. However, this section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
6544	6	85	14		15	Modify the Description, taking into consideration that the chance to fail in meeting the 450 ppm CO2-equv target of models is high if some of the mitigation technologies are not available, according to the descriptions of P48 line 11-16 and Figure 6.29.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
8054	6	85	14	85	15	I read: 'Reductions in the availability of mitigation technologies can also reduce costs, more than doubling costs when key technologies such as CCS are not available'. I thought the opposite: reductions of the availability of technologies leads to an increase of costs: 'Reductions in the availability of mitigation technologies can also increase costs, more than doubling costs when key technologies such as CCS are not available'.	Accepted. However, this section is going to be substantially shortened for space, so very little will remain from what is currently there. Nonetheless, the point is being considered in the current draft.
14406	6	85	3			The missing figures (“xx%”) are crucial!	Noted.
6542	6	85	2		4	Modify the description, taking into consideration that the macroeconomic costs to meet goals below 2.6W/m2 given here may have serious downward bias, as suggested in P48 line 11-16 and P56 line 25-28.	Noted.
14034	6	9	1		8	The large-scale transformations in human society will undoubtedly involve much more than how we produce and consume energy and use land; it will involve both those large and visible changes in systems and structures, but it will involve also change in meaning making and in the way society relates to nature (see comment #37). It would be good if the introduction made it clear what changes the chapter focuses on and how this relates to the wider literature on social change and transformation.	Noted.
11370	6	9	17	9	22	Literature: Here references should be provided	Noted. Referencing will be improved in the SOD.
4191	6	9	17	10	6	I agree these are important point. But I think this paragraph can be written concisely focusing on the variety of possible options including mitigation and adaptation and trade-offs among factors.	Editorial.
3073	6	9	19	9	29	Same comment as p. 5 lines 3, above	Noted. Could not find the previous comment.
13128	6	9	2	9	2	Replace "will" with "would". Few things currently suggests that we are on that trajectory.	Noted. The introduction is being revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified.
16687	6	9	21			after the word "choices", insert "made over decades" to as to reinforce that this is a long term process and choices and pathways evolve over time. It may help to make this explicit and state that pathways may evolve over time as we learn and as conditions change ... the path will be adjusted. This policy is unlikely to be set and then never adjusted -- we will have the option to go faster or slower.	Noted.
14453	6	9	3	9	4	CO2 emissions can never be zero, all animals respire CO2 and organic matter decomposes. Do you mean net fossil-fuel and LUC derived CO2 emission must be zero? This concept must be edited for accuracy.	Noted. The introduction is being revised and the ordering and nature of points that it makes will be different in the SOD. If this statement remains, it will be clarified.
9279	6	9	33	9	43	This paragraph is duplicative of Chapter 6, page 5 lines 17 - 27.	Noted. In the SOD, overlaps between the ES and the introduction wiill be revisited.
16688	6	9	33		43	Redundant with previous paragraph? Delete?	Noted.
14395	6	9	4			Reiteration of zero emissions target is very unfortunate by giving the impression that it is impossible to achieve and tilting the policy mix toward (risky) geoengineering.	Noted.
14035	6	9	40			Add equity concerns	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
12624	6	9	41	9	43	I see no reason to single out any technologies here. All technologies include trade offs, CCS, Nuclear, Wind, Solar, etc.	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
12667	6	9	41	9	43	I see no reason to single out any technologies here. All technologies include trade offs, CCS, Nuclear, Wind, Solar, etc.	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
9566	6	9	42			Please, remove coal-fired from coal-fired CCS as we need any types of CCS.	Noted.
9565	6	9	42	9	43	Please, delete examples of nuclear and CCS, or add examples of wind power and geothermal as they involve bird strikes (wind power) and sources of mercury contamination (geothermal power).	Noted. The questiion of when to single out technologies as examples is being considered for the SOD.
9837	6	9	45			I think that this issue of "feasibility" is raised far too early in the chapter, and too much emphasis is given to it. You have not even described many other key results yet, and infeasibility is not very interesting as an issue since it is purely an artifact of the limited range of input assumptions used in some models.	Noted. There is some confusion between the notion of feasibililty in general and the concept of models not being able to produce particular scenarios. This will be clarified in the SOD.
8623	6	9	45			I think that this issue of "feasibility" is raised far too early in the chapter, and too much emphasis is given to it. You have not even described many other key results yet, and infeasibility is not very interesting as an issue since it is purely an artifact of the limited range of input assumptions used in some models.	Please see the response to comment 9837, which appears to be a duplicate of this comment, despite being submitted by another reviewer.
15280	6	9	47	9	47	"biogoephysical" to be "biogeophysical"	Editorial.
12307	6	9	9	9	16	Please consider to put this passage first in the introduction.	Noted.
8973	6	90	7			Add the following reference. Fleming, J.R. (2010) Fixing the Sky: The checkered history of weather and climate control. Columbia University Press, New York. 325 pp.	Noted. We will consider this reference.
14539	6	all				No mention is made of '350 ppm' target which the Association of Small Island States is asking for. Although the Copenhagen agreement called for a 450 ppm target, it also said that '350 ppm' would be considered	Accepted. We will mention the 350 ppmv CO2-e goal in the SOD.
11996	6	all				I strongly suggest to acknowledge the achievements of the CDM in this chapter, it currently is not mentioned even once. The success to date and potential cost mitigation potential, signalling effect, contribution to improved justice and perceived fairness as well as the training provided so far is all very well documented in the study commissioned by the UNFCCC's CDM Executive Board, overviewed by an independent High Level Policy Panel. The findings and underlying research can be found on cdmpolicydialogue.org.	Rejected. This is not the chapter for discussion of CDM. This chapter is considering long-term transformation pathways. CDM would be more appropriately discussed in the policy chapters.
2185	6	`65	28	67	36	It would be useful to introduce some concepts here that the climate community may be unfamiliar with such as sustainability, sustainable development (as it pertains to all countries not just developing countries), wellbeing. Without these concepts it is hard to explain why integration of climate mitigation within broader sustainable development goals is important. Mention should also be made of the commitment at Rio+20 to develop a set of sustainable develpment goals by 2016, in which climate mitigation is certain to be included.	Noted. This section has been significantly restructured with the new draft.